THURSDAY, JANUARY 23, 1896.

SOME RECENT WORKS ON BUTTERFLIES AND MOTHS.

A Handbook of British Lepidoptera. By Edward Meyrick, B.A., F.Z.S., F.L.S., F.E.S., Assistant Master at Marlborough College. (London: Macmillan and Co., 1895.)

British and European Butterflies and Moths (Macro-Lepidoptera). By A. W. Kappel, F.L.S., F.E.S. (Assistant Librarian, Linnean Society), and W. Egmont Kirby, L.S.A. With thirty coloured plates by H. Deuchert and S. Slocombe. (London: Ernest Nister, 1895.)

Die Artbildung und Verwandschaft bei den Schmetterlingen. II. Theil. Eine systematische Darstellung der Abänderungen, Abarten, und Arten der Schwalbenschwanz-ähnlichen Formen der Gattung Papilio. Von Dr. G. H. Theodor Eimer, Professor der Zoologie und vergleichenden Anatomie zu Tübingen. Unter Mitwirkung von Dr. K. Fickert, I. Assistent an der Zoologischen Anstalt daselbst. Mit 4 Tafeln in Farbendruck und 7 Abbildungen im Texte. (Jena: Gustav Fischer, 1895.)

LAST year was remarkable for the unusual number of publications on *Lepidoptera* which were issued from the press, especially in England. In addition to serial works and reprints, many independent works appeared, several of which have already been noticed in the columns of NATURE; and the three new books to which the present article refers, shows that the activity of entomologists in this direction is still as great as ever.

It is singular that, notwithstanding the great increase of interest taken in the British *Lepidoptera* of late years, Mr. Meyrick's is really the first serious attempt at a student's manual of the order which has yet appeared. We have any amount of popular illustrated books; but nothing beyond, except Stainton's "Manual of British Butterflies and Moths," a book nearly forty years old (though still of great value and much used, especially as it gave the only available synopsis of the *Micro-Lepidoptera*, which are generally neglected in popular works), and written rather for beginners than for students. Mr. Meyrick now offers us, in a compact volume of nearly 850 pages, a series of carefully drawn up descriptions of genera and species, with notices of localities, range, transformations, &c., and numerous systematic and phylogenetic tables; but there is not a trace of popular padding, and no illustrations, except occasional woodcuts of neuration.

As is known to all who have examined Mr. Meyrick's previous entomological writings (which have chiefly been devoted to the *Lepidoptera* of Australia and New Zealand), he attaches great importance to the neuration of the wings of the insects; and in the present volume he has adopted a new classification of his own, based mainly on the wing-neuration, which will appear to most of his readers little short of revolutionary. Suffice it to say that he divides the *Lepidoptera* into nine main groups, as follows: (1) *Caradrinina*, including the bulk of the species generally classed under *Arctiidæ*, *Liparidæ*, and part of *Noctuæ*; (2) *Notodontina*, including, in addition to the *Notodontidæ*, the *Saturniadæ*, *Sphingidæ*, *Geometræ*, and part of *Noctuæ*; (3) *Lasiocampina*, including also the *Drepanulidæ* and *Endromididæ*; (4) *Papilionina*, including the butterflies; (5) *Pyralidina* (from which group Mr. Meyrick believes the butterflies have branched), including also the Crambi and Plume moths; (6) *Psychina*, with which are included the *Zeuzeridæ*, *Zygænidæ*, and *Heterogenidæ*; (7) *Tortricina*, with which is included *Trypanus cossus*, L.; (8) *Tineina*, including also *Ægeriadæ*; and (9) *Micropterygina*, including the *Hepialidæ* and *Micropterygidæ*. Comment is needless; every one who knows our British *Lepidoptera* will recognise the sweeping character of the changes proposed; and how far they will ultimately be accepted, no one can venture to predict at present.

We will now turn to the second book on our list, which is a complete contrast to the last in design and execution. It is a handsomely got-up book intended for popular use, and illustrated with a series of excellent coloured plates, representing a considerable number of the more interesting and conspicuous butterflies and larger moths of Great Britain and the adjacent parts of Europe. All, or nearly all, the species not figured, but which fall within the limits of the book, are described in the text. No innovations of arrangement or nomenclature are attempted; and English as well as Latin names are used throughout. The book extends to the *Geometræ* inclusive; but it is more fully illustrated in the earlier groups than in the later. Useful information on technical terms, collecting and preserving, and other general matters connected with *Lepidoptera*, will be found in the introduction. This book may be safely recommended as a desirable addition to school libraries, or public institutions, or as a present to a young friend interested in natural history.

The last book on our list is one of a very different kind to the others. It is an elaborate philosophical study of the various forms presented by three groups of swallow-tailed butterflies, represented by *Papilio Turnus*, *Machaon*, and *Asterias*, in various parts of their range. These are mainly Palæarctic and Nearctic in their range. In the first volume of his work, published in 1889, Dr Eimer discussed the groups represented by *Papilio Podalirius*, *Antiphates*, *Anticrates*, *Ajax*, and *Policenes*, which are chiefly East Indian or African, though *P. Ajax* and its allies are American.

Dr. Eimer has come to the conclusion that natural selection will not account for the origin of species, though it may largely contribute to their preservation when already formed. This he expresses in a rather polemical preface, in which he claims to have practically demolished natural selection in the former sense. Perhaps it will be most fair to him to quote a paragraph, especially as it contains most of the technical terms which he uses in his work.

"In complete contrast to the teachings of Darwin, my butterflies everywhere display the origin of new peculiarities through orderly development in a few definite directions (Orthogenesis) on the basis of physiological causes, by organic increase (Organophysis). They show that it is really Genepistasis, or arrest of development at definite stages, which necessitates the separation into species of the organic chain which has

thus originated, and which is, in fact, the cause of the origin of species, independently of, and apart from other causes, discussed in the following pages, such as impediments to fertilisation (Kyesamechanism), and sudden development (Halmatogenesis)."

It is probable that various causes, acting unequally in different groups of animals and plants, have contributed to the origin and differentiation of species; and there is certainly much to be said in favour of the view that evolution proceeds along definite lines, and also that it is liable to be arrested at certain definite points. With respect to the latter phenomenon, we may refer to Dr. Leuthner's monograph of the *Odontolobidæ*, a family of East Indian stagbeetles, published in the *Transactions* of the Zoological Society for 1886, which illustrates this very clearly. It may also be observed in the common earwig.

Besides the discussion of the various points noted in the above-quoted paragraph, this work contains incidental remarks on the influence of heat and cold on butterflies reared artificially; and on mimicry, &c., as well as a reply to some criticisms by the late Dr. Haase on the first volume of the work.

While the philosophical part of the book will be read with interest by students of evolution, the special part, with its excellent illustrations of closely allied species and varieties, will be useful to collectors of foreign butterflies generally.

RECENT WORKS ON PHYSIOLOGY.

A Manual of Physiology. By G. N. Stewart, M.A., D.Sc., M.D.Edin. (London: Ballière, Tindall, and Cox, 1895.)

Physiology. By A. Macalister, LL.D., M.D., F.R.S. "Manuals of Science Series." (London: Society for Promoting Christian Knowledge, 1895.)

Elementary Physiology. By J. R. Ainsworth Davis, B.A. (London: Blackie and Son, 1895.)

WE have at the present time several very excellent text-books of physiology in English, adapted for the various needs of different classes of students. And to prevent these being inconveniently increased, it is a matter of tacit agreement amongst teachers of physiology that it is undesirable to afford a welcome to any new text-book, unless it has itself the highest claims to recognition. These claims may be based either upon some novel method of treatment of the subject, or upon the impression on the work of an author's high personal authority.

Adopting such a standard of criticism as the above, we first proceed to consider Dr. Stewart's manual. The fact of the author possessing so brilliant a reputation in the application of physics to physiology, is sufficient to make a work at his hands of the utmost value in certain branches of the subject. There are other influences to which Dr. Stewart has been exposed, which also tend to enhance the value of his work. The teaching methods of the Edinburgh school involve both conciseness and dogmatism, and these are as desirable in elementary nstruction as they are pernicious in more advanced.

The manual which Dr. Stewart has written is to be regarded as an elementary *text-book*, for it is really something more than a mere *manual*. The methods throughout involve a concise expression of varying views, and the author's personal preference for any particular view is clearly indicated. Such a method we consider very desirable in a book of this character. As regards the facts incorporated, they include in general the very latest work that the time of publication permitted, and indicate an acquaintance with the progress of physiology as is possible only with a specialist in that branch of science. Interwoven with the academic exposition are detailed instructions for practical work. We have not very much faith in the success of this innovation. It is extremely inconvenient for a student to have to carry from his home to his laboratory a somewhat bulky volume, and the instructions are of no value except in the laboratory. We certainly think that it would have been much better to have omitted from such a book as this all practical instruction. We would like to offer some criticism also upon this practical instruction. It is to a considerable extent of such a character as to be impossible even to an American student, though Dr. Stewart dwells on the greater opportunities existing on the other side of the Atlantic for practical investigation. We have some doubts, also, concerning the advisability of introducing the so-called electro-physiology to the extent that has been done. It is not that too many pages have been allotted to it; on the contrary, if introduced at all, it requires many more than the author saw his way to give. The result is that, though so much has been referred to, it has been treated so briefly that the fault of brevity, vagueness, is only too apparent. We think that few students will derive the proper amount of information from this section unless it be largely supplemented by the personal instruction of a teacher.

For certain classes of students, however, the book will be of special service, and we think that it will undoubtedly be regarded as a valuable addition to our present text-books of physiology.

For Prof. Macalister's little manual we have nothing but praise. So much of elementary physiology as it is generally understood is identical with elementary anatomy that the physiologist is not at more advantage than the anatomist in describing the more elementary facts. A somewhat novel arrangement of the different points treated upon has been adopted. The author has commenced by describing the nature of food and digestion. This is already a matter of some slight acquaintance to one who has not studied physiology, and he is thus led on through partially familiar paths.

Sections then follow, treating of the blood, the circulatory system, respiration, the skeleton, joints, the nervous system, and the organs of the senses. A small amount of histology has also been introduced. The author throughout the book teaches valuable hygienic lessons from the explained phenomena. In a later edition a few corrections might be introduced; and when that is done, the book will be an excellent guide to the explanation of elementary physiological principles.

Mr. J. R. Ainsworth Davis's work is intended by the author (1) for elementary students in general; (2) for students of general biology who wish to supplement the subject on the physiological side; (3) students of psychology; (4) students of agriculture and dairying.

The necessities of these different classes of students had carefully to be borne in mind in writing the book, and consequently a work of somewhat peculiar character has been compiled. We think it undesirable that the aim of an author should be quite so far-reaching. The extensiveness is bound to detract from the value of the work for any particular class. As regards the matter actually in the volume, it is more elementary in character than that of Prof. Macalister's little book, though occupying about double the space. A redeeming feature of the work is the illustrations, which are judiciously chosen and very well executed. J. S. EDKINS.

OUR BOOK SHELF.

Practical Plane and Solid Geometry. By Joseph Harrison, M.I.M.E., A.M.I.C.E., and G. A. Baxandall. Pp. vi + 183. (London: Macmillan and Co., 1895.)

PRACTICAL geometry is taught primarily as an aid to systematic, accurate and rapid delineation of concrete objects, and, in most cases, it forms part of the student's work very early in his scientific or technical training.

In the organised science school the subject is taken contemporaneously with the first or first two books of Euclid; while the student in a technical school almost invariably goes through his elementary course of practical geometry before he arrives on the other side of the *pons asinorum*. This seems to be hardly logical, for the more important problems in practical plane geometry to be found in elementary text-books depend on propositions in Euclid's third and sixth books; and the early problems in solid geometry are based on theorems in the eleventh book; but in the majority of technical classes it is in the nature of things that practical geometry must be taught where Euclid's sixth and eleventh books are practically sealed. It is imperative that every student should be satisfied with the foundation upon which his future work is to be built; and the truth of theorems, which have such general application as that of similar triangles, must be made apparent at the outset; and with beginners, this can best be done experimentally. It is rarely that the author of a text-book is bold enough to advocate the experimental demonstration of the truth of a law in geometry, though that is almost the only method in many other branches of science.

In the little work before us, Messrs. Harrison and Baxandall have so far deviated from the beaten track as to get through a good deal of their work without some of the usual cumbrous and, to beginners, unintelligible definitions; and have added, in the form of an appendix, some theorems and definitions that may be found useful in a second reading. Some new problems have been introduced in the first and second chapters, which are devoted to plane geometry and graphic arithmetic respectively. The authors have wisely condensed the whole of the former into one chapter; and though one may be considered pedantic for saying so, there are still left in some problems that can hardly be called by the first word of the title-page. It is with great satisfaction that we notice the absence of any attempt to introduce statics with the graphic arithmetic. Too much can hardly be said in favour of graphic statics in its proper place; but when taught without any reference to the laws of equilibrium, it is not only useless but harmful.

The student is gradually led on to the study of solid geometry by a series of well-chosen diagrams and figures, and becomes well-grounded in the orthogonal projection of solids before he approaches the usual oblique line and plane problems. The figures in the text are bold, clear, plentiful and well-lettered; and this is a feature which, if lost sight of, causes much inconvenience and annoyance.

Altogether this is a work which is very carefully compiled, and from which a student can extract for himself a great deal of information without outside assistance.

A Treatise on Hydraulics. By Prof. Henry T. Bovey. Pp. viii + 336. (New York: John Wiley and Sons. London: Chapman and Hall, 1895.)

THE contents of this book formed the subject-matter of a series of lectures on hydraulics, delivered by the author at McGill University, Montreal; and though chiefly intended for students, it will doubtless prove valuable for reference to hydraulic engineers. The book deals with the experimental theory of the motion of water, its flow in pipes and open channels, and its practical application to hydraulic motors. The subject is divided into seven chapters, treating successively of flow through orifices and over weirs, fluid friction, flow in pipes, flow of water in open channels, methods of gauging, impact, and hydraulic motors and centrifugal pumps. The matter is, for the most part, treated mathematically; and the book will be mainly useful to persons who have studied mathematics; whilst numerous examples are appended at the end of most of the chapters, together with their answers, which will give students facility in working out hydraulic problems, and enable them to acquire a more thorough grasp of the principles enunciated. One hundred and ninety-six simple diagrams and drawings serve to illustrate and elucidate the text; and the book, being clearly and concisely written, will enable the intelligent reader, with some knowledge of mathematics, to obtain a deeper insight into the theory of hydraulics, and the principles of its application to practical purposes. The short chapter on gauging the velocity of flow in open channels, describes briefly and practically the different instruments used for the purpose; whilst the long final chapter, on hydraulic motors, explains, with the aid of formulæ, the principles involved in the hydraulic ram, pressure-engines, the accumulator, hydraulic brakes, the various forms of water-wheels and turbines, and centrifugal pumps.

The Scientific Foundations of Analytical Chemistry. By Wilhelm Ostwald, Ph.D. Translated by George McGowan, Ph.D. (London: Macmillan, 1895.)

THE peculiarly interesting character of this book has already been pointed out in the review of the German edition (see NATURE, vol. li. p. 482), and Dr. McGowan's translation now places its doctrines within the reach of English readers.

The only alteration made by the author upon the original edition, is the introduction of a portion of a chapter on the theory of electrolytic methods of analysis. This includes an account of the mode in which the processes of oxidation and reduction occurring in voltaic cells, and the order in which metals separate from a mixture of electrolytes under the influence of a definite external electromotive force, can be correlated with the potential differences given by the metals and the electrolytes used. J. W. R.

Elementary Algebra. By J. W. Welsford, M.A., and C. H. P. Mayo, M.A. Pp. xiii + 407. (London: Longmans, Green, and Co., 1895.)

SINCE, in its early stages, algebra is a generalisation of arithmetic, the authors give a short account of some of the processes of arithmetic, as an introduction to their subject. The book differs in character and arrangement from most text-books on algebra: for instance, the radical sign is explained at the same time as the positive integral index; factors are freely used; a table of logarithms is introduced; and oral exercises are given. There are about four thousand examples to test the student's knowledge, and impress the book-work upon his memory.

LETTERS TO THE EDITOR.

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On Röntgen's Rays.

PROF. RÖNTGEN'S remarkable discovery will materially affect our views concerning the relation between the ether and matter; but further experimental evidence is required before any opinion can be expressed as to the character of the rays, which behave in so straightforward a manner that they seem to upset all one's notions of the laws of nature. Prof. Röntgen, on the strength of his carefully-conducted experiments, has arrived at a conclusion adverse to the idea that the rays only differ from light rays by the smallness of wave-length. Perhaps the following considerations may show that the evidence is not conclusive in this respect.

Röntgen's rays are not kathode rays—there can be no doubt on that point—but they are generated at the point of impact between the kathode ray and solid substances.

The discoverer has not been able to obtain any interference effects, possibly, as he says, owing to the weakness of the radiation. An absence of interference would not, however, be sufficient to show that the radiation is not of the nature of ordinary light, but only that it does not possess sufficient regularity, or, in other words, that the disturbance is not sufficiently homogeneous. That this is the case is not at all improbable, for the radiation is produced by an impact, which in the first instance may be an impulsive motion propagated outwards, and after passing through the screen, would only possess such regularity as is impressed on it by the absorption of the longer waves.

The great argument against the supposition of waves of very small length lies in the absence of refraction; but is this conclusive?

When we speak of the size of the atoms, we mean their distance in the solid and liquid state. The properties of the ether may remain unaltered within the greater part of the sphere of action of a molecule. The number of molecules lying within a wave-length of ordinary light is not greater than the number of motes which lie within a sound-wave, but, as far as I know, the velocity of sound is not materially affected by the presence of dust in the air. Hence there seems nothing impossible in the supposition that light-waves, smaller than those we know of, may traverse solids with the same velocity as a vacuum. We know that absorption bands greatly affect the refractive index in neighbouring regions; and as probably the whole question of refraction resolves itself into one of resonance effects, the rate of propagation of waves of very small lengths does not seem to me to be pre-judged by our present knowledge. If Röntgen's rays contain waves of very small length, the vibrations in the molecule which respond to them would seem to be of a different order of magnitude from those so far known. Possibly we have here the vibration of the electron within the molecule, instead of that of the molecule carrying with it that of the electron.

I should like, further, to express a certain sense of satisfaction that Röntgen's rays are not deflected in a magnetic field. They are thus clearly separated from kathode rays. The idea that kathode rays are due to vibrations has become fashionable; yet the fact that the magnet deflects them just as it would an electrified molecule, has always seemed to me to be conclusive against this view. No one has, so far, given any plausible reason why a ray of *invisible* light should be able to run round in a spiral, while a ray of *visible* light goes straight; and, so far, Röntgen's rays behave as we should expect well-conducted vibrations to do.

It is not my intention to argue in favour of any particular theory, or against Röntgen's suggestion that we have at last found the formerly missed longitudinal wave. I only desire to put those points forward which at first sight seem to go against the supposition of ordinary light vibrations, and to raise the question whether they constitute an insuperable difficulty.

ARTHUR SCHUSTER.

IN connection with the wonderful discovery by Prof. Röntgen of photographic rays, apparently hitherto unknown, and in connection with the speculation which concludes Prof. Röntgen's most interesting paper, that these rays may perhaps be longitudinal vibrations of the luminiferous ether, the following extracts will probably be found of interest to the readers of NATURE. They are taken, by permission of Lord Kelvin, from his Baltimore Lectures, delivered at the Johns Hopkins University in 1884.

The first extract is from the reprint (now in progress) of Lecture IV. Referring to mathematical work immediately preceding, Lord Kelvin says:—". . . We can do that [obtain certain forms of solutions of equations] for the purpose of illustrating different problems in sound, and in order to familiarise you with the wave that may exist along with the wave of distortion in any true elastic solid which is not incompressible. We ignore this condensational wave in the theory of light. We are sure that its energy, at all events, if it is not null, is very small in comparison with the energy of the luminiferous vibrations we are dealing with. But to say that it is absolutely null, would be an assumption that we have no right to make. When we look through the little universe that we know, and think of the transmission of electrical force, and of the transmission of magnetic force and of the transmission of light, we have no right to assume that there may not be something else that our philosophy does not dream of. We have no right to assume that there may not be condensational waves in the luminiferous ether. We only do know that any vibrations of this kind, which are excited by the reflection and refraction of light, are certainly of very small energy compared with the energy of the light from which they proceed. The fact of the case as regards reflection and refraction is this, that unless the luminiferous ether is absolutely incompressible, the reflection and refraction of light must generally give rise to waves of condensation. Waves of distortion may exist without waves of condensation, but waves of distortion cannot be reflected at the bounding surface between two mediums without exciting in each medium a wave of condensation. When we come to the subject of reflection and refraction, we shall see how to deal with these condensational waves, and find how easy it is to get quit of them by supposing the medium to be incompressible. But it is always to be kept in mind as to be examined into, are there or are there not very small amounts of condensational waves generated in reflection and refraction, and may, after all, the propagation of electric force be by these waves of condensation?

"Suppose that we have at any place in air, or in luminiferous ether (I cannot distinguish now between the two ideas) a body that, through some action we need not describe, but which is conceivable, is alternatively positively and negatively electrified; may it not be that this will give rise to condensational waves? Suppose, for example, that we have two spherical conductors united by a fine wire, and that an alternating electromotive force is produced in that fine wire, for instance by an 'alternate current' dynamo-electric machine; and suppose that sort of thing goes on away from all other disturbance—at a great distance up in the air, for example. The result of the action of the dynamo-electric machine will be that one conductor will be alternately positively and negatively electrified, and the other conductor negatively and positively electrified. It is perfectly certain, if we turn the machine slowly, that in the air in the neighbourhood of the conductors we shall have alternately positively and negatively directed electric force with reversals of, for example, two or three hundred per second of time with a gradual transition from negative through zero to positive, and so on; and the same thing all through space; and we can tell exactly what the potential and what the electric force is at each instant at any point. Now, does any one believe that, if that revolution were made fast enough, the electro-static law of force, pure and simple, would apply to the air at different distances from each globe? Every one believes that if that process be conducted fast enough, several million times, or millions of million times per second, we should have large deviations from the electro-static law in the distribution of electric force through the air in the neighbourhood. It seems absolutely certain that such an action as that going on would give rise to electrical waves. Now it does seem to me probable that those electrical waves are condensational waves in luminiferous ether; and probably it would be that the propagation of these waves would be enormously faster than the propagation of ordinary light waves.

"I am quite conscious, when speaking of this, of what has been done in the so-called electro-magnetic theory of light. I know the propagation of electric impulse along an insulated wire surrounded by gutta-percha, which I worked out

myself about the year 1854, and in which I found a velocity comparable with the velocity of light. We did not then know the relation between electro-static and electro-magnetic units. If we work that out for the case of air instead of gutta-percha we get simply v (that is, the number of electro-static units in the electro-magnetic unit of quantity) for the velocity of propagation of the impulse. That is a very different case from this very rapidly varying electrification I have ideally put before you, and I have waited in vain to see how we can get any justification of the way of putting the idea of electric and magnetic waves in the so-called electro-magnetic theory of light.

"I may refer to a little article of mine in which I gave a sort of mechanical representation of electric, magnetic, and galvanic forces—galvanic force I called it then, a very badly-chosen name. It is published in the first volume of the reprint of my papers. It is shown in that paper that the static displacement of an elastic solid follows exactly the laws of the electro-static force, and that rotatory displacement of the medium follows exactly the laws of magnetic force. It seems to me that an incorporation of the theory of the propagation of electric and magnetic disturbances with the wave theory of light is most probably to be arrived at by trying to see clearly the view that I am now indicating. In the wave theory of light, however, we shall simply suppose the resistance to compression of the luminiferous ether, and the velocity of propagation of the condensational wave in it, to be infinite. We shall sometimes use the words 'practically infinite' to guard against supposing these quantities to be absolutely infinite."

The second extract which I give is from p. 143 of the Papyrograph edition of the "Baltimore Lectures"—a portion not yet reprinted.

"The want of indication of any such actions is sufficient to prove that if there are any in nature, they must be exceedingly small. But that there are such waves, I believe, and I believe that the velocity of propagation of electro-static force is the unknown condensational velocity that we are speaking of.

"I say 'believe' here in a somewhat modified manner. I do not mean that I believe this as a matter of religious faith, but rather as a matter of strong scientific probability."

J. T. BOTTOMLEY.

13 University Gardens, Glasgow, January 16.

The Astronomical Theory of the Ice Age.

MAY I first acknowledge the gentle kindliness with which my early teacher and friend, Sir Robert Ball, has pointed out my error in quoting from the old edition of his work. I much regret that I did not make further inquiries, but I was satisfied when the library clerks at Trinity College, Dublin, told me that if there had been any alteration in the text, they would have received a copy of the second edition. It appeared from Sir H. Howorth's letter that the mistake originated with the publishers, who erroneously informed the library agent that the second edition was a mere reprint, and therefore refused to supply a copy.

Both Sir Robert Ball and Dr. Wallace, in their letters in NATURE of January 9, have misunderstood the way in which I present my argument. If Dr. Wallace would read my papers again, I think he will see that, so far as I am concerned, the whole of his letter is founded on a complete misapprehension; and Sir Robert Ball will, I hope, also agree that he has somewhat altered the form in which I have stated my conclusions, and that I have fully recognised the difference which he thinks I have ignored. But as the matter really at issue is the present position of the astronomical theory, I may be excused from discussing this misunderstanding further, for even if every word of their criticisms on my conclusions were valid, the astronomical theory, *as it issued from the labours of Croll and Ball*, would be in no better position than before. Whether I am right or wrong in my belief that the astronomical factor cannot have been the principal one, I venture to think there can be no doubt that the existing exposition of that theory must be given up.

The foundation of the astronomical theory is the fall in temperature *directly* due to diminished sun-heat. Croll and Ball accordingly give calculations which indicate a large fall. Croll gets 45·3° F. for the lowering of mid-winter temperature in Great Britain during the long excentric winter, and Ball's modification of Croll's method gives about 25° F. as the lowering of the winter temperature. The first five pages of my article in the *Phil. Mag.* for December 1894 are devoted to showing that there is no justification for the principle on which this calculation is made, and that the fall must be a mere fraction of that postulated in either exposition of the astronomical theory. The chief flaw in the calculation is, curiously enough, that which Sir Robert and Dr. Wallace erroneously attribute to me, viz. that of considering that changes in terrestrial temperature are directly proportional to the changes in sun-heat, and ignoring the important element of storage and transference by ocean and air currents. How unsafe this is may be judged from the fact that if the method used to calculate the temperature in the Glacial Age from that in the present day were applied to find the summer temperature from the winter temperature, we should find for the British Isles a summer temperature of above 300° F. if we take Ball's hypothesis, and some thousands of degrees Fahrenheit if we take Croll's. If we calculate the winter temperature from the summer one, we should get −125° F. for our winter temperature. A method which gives results in such striking contrast to the truth can hardly be accepted as a basis for a scientific theory.

If, therefore, this first portion of my criticism be correct (and hitherto no attempt has been made to refute it) the astronomical hypothesis is in just the position it would occupy if neither Croll's nor Ball's book had been written. So far, *the hypothesis itself* may be true or false; it is only the *reasoning* which has been put forward in its support that has to be abandoned or modified. The theory is, as all will admit, a tempting one, and accordingly I sought for some other means of establishing it. After several fruitless efforts to hit on a fairly satisfactory method of estimating the *direct* effect of an altered distribution of sun-heat on terrestrial temperatures, the method which Prof. Darwin has described occurred to me, and from it, *combined with a discussion on the transference of heat by the Gulf Stream* (see *Phil. Mag.* December 1894, p. 548 and p. 551), I was led to infer that for the British Isles at least the glaciation could not with any degree of probability be attributed to the long winter of great excentricity.

Sir Robert Ball's views, as presented in his letter, seems to involve a return to Croll's point of view, at least to the extent that the purely astronomical reason requires to be supplemented by a discussion of the oceanic and atmospheric currents. This view appears to me a true one; the only hope for the astronomical theory would be to show that the adjustment of terrestrial temperatures by the interaction of ocean and air currents with direct sun-heat is such that a very slight alteration of sun-heat produces a very great alteration of temperature; so that if the sun-heat which falls on Cornwall in winter were to be reduced to that which falls on Yorkshire, with corresponding changes for the temperate latitudes, and somewhat greater ones for the tropical belt, the ultimate result would be an Ice Age. But how can we hope to establish such a theory when we remember what a comparatively small change of temperature is due to the far greater changes of sun-heat from equator to pole as summer gives way to winter? EDWD. P. CULVERWELL.

Trinity College, Dublin, January 14.

Changes of Length in Bars and Wires of Magnetic Material due to Magnetisation.

THE appearance of a paper, by Dr. L. T. More, on the changes in length produced in iron wires by magnetisation (*Phil. Mag.*, October 1895, p. 345, and *Physical Review*, vol. iii. p. 210), has drawn my attention to a curious divergence of opinion on a fundamental point in magnetism. Dr. More has attempted to analyse the change of length accompanying magnetisation into a "*direct action*" which "may possibly be due to the orienting of the magnetised particles," and to "indirect actions." He adds (p. 224): "These indirect actions are the mechanical stresses created in the rod by the magnetism. The first of these . . . is the tractive force of the magnet and is measured by $B^2/8\pi$. That this force exists, tending always to *contract* the rod (italics mine), is seen from the fact that if the magnet is cut in two, the ends are held together. . . . This effect for high intensities of magnetisation is a large one, and becomes one of the most important factors in the observed changes in length." The stress referred to by Dr. More is that usually associated with the name of Maxwell ("Electricity and Magnetism," vol. ii. Arts. 641 *et seq. Cf.* Ewing, "Magnetic Induction in Iron and other Metals," § 147).

The first to propound the view adopted by Dr. More—that the mechanical force tends to *shorten* the rod—would seem to be

Mr. Shelford Bidwell (*Phil. Trans.*, 1888, pp. 216-7). He has been followed by Prof. Ewing, on pp. 242-3 of his "Magnetic Induction."

On the other hand, Maxwell, *l.c.*, speaks of a *tension* along the lines of force, and even more explicit statements to the same effect are due to Kirchhoff (*Wied. Ann.*, Bd. 24, p. 52, and Bd. 25, p. 601) and Prof. J. J. Thomson ("Applications of Dynamics to Physics and Chemistry," p. 52). The latter says: ". . . there are the strains arising from Maxwell's distribution of stress. Kirchhoff has investigated the effect of this on a small soft iron sphere placed in a uniform magnetic field and has shown that it would produce an *elongation* . . . along the lines of force and a contraction at right angles to them. We may therefore assume that in general this distribution of stress causes an *expansion* . . . in the direction of the lines of force . . ." (italics mine).

Prof. Thomson's view was accepted and restated by myself in discussing the phenomena in cobalt (*Phil. Trans.*, 1890, p. 339). The fact that my views escaped Dr. More's notice is not surprising, but it is strange that he overlooked Prof. Thomson's statement, and that I overlooked Mr. Shelford Bidwell's. My belief now, as before, is that the view of Kirchhoff and J. J. Thomson is the correct one.

Let us take the case of a long iron bar magnet suspended vertically. Suppose it cut in two transversely at a section A; then, supposing the magnetism sufficiently intense, the lower portion will, we know, adhere to the upper until a considerable weight, W, is hung from the lower end, B. If w be the weight of AB, the magnetic force over A must balance $W + w$, acting downwards, and so must itself act upwards, or away from AB. In other words, the magnetic force across A acts precisely like the elastic stress across a section of a rope fastened at one end and pulled at the other. The rope, as every one will realise, is under tension not pressure, and so clearly is the iron bar.

A more general way of looking at the matter is as follows:

$$\begin{array}{cccc} A' & A & B & B' \\ -m\,| & |\,+m & -m\,| & |\,+m \end{array}$$

Take imaginary sections A and B of a long uniformly magnetised straight or bent bar, and regard coterminous surfaces AA′ and BB′ as separated by distances small compared to AB. Taking the usual elementary theory, we see the force acting on the distribution $+m$ over A is $F - F' + F''$ to the *left*, where F arises from $-m$ over A′, $-F'$ from $-m$ over B, and F'' from $+m$ over B′. Now clearly F′ and F″ are approximately equal, and each is small compared to F, so that A is obviously pulled to the left. Similarly B is pulled to the right, or AB is under tension.

Of course if the bar, when supported horizontally on a frictionless table, were actually cut through at A, foreign material—a finger, for instance—inserted between the naturally coterminous surfaces A and A′ would experience compression. I cannot avoid the conclusion that Mr. Shelford Bidwell and Dr. More have fallen into confusion through regarding the problem from this point of view, forgetting that the surfaces supposed to attract one another have nothing between them to squeeze.

That the question is of vital importance to a true understanding of the phenomena in iron is unquestionable if Dr. More's experiments—which seem carefully designed—gave anything like the true results. A reversal of the sign in his correction $B^2\delta l/8\pi l$ in his tables I. to IV.—necessitated by the view I advocate—would completely alter the character of the physical deductions to be made. The occasion for the correction, I may add, is as clearly indicated by Mr. Shelford Bidwell as by Dr. More, but the experimental data obtained by the former led him to attach less importance to it.

It is not my intention to discuss the actual magnitude of the magnetic stress or of the consequent alteration in length. An interesting experimental paper on the former subject by Mr. E. Taylor Jones, with references to recent conflicting authorities, appeared in the *Phil. Mag.*, March 1895, pp. 254-267. I may say, however, that the theoretical importance of investigations such as those of Prof. J. J. Thomson into the mutual relationships of magnetism and elasticity renders it all the more desirable that any doubt as to the true nature of the experimental results should be removed. To be altogether satisfactory, these investigations require trustworthy magnetic and elastic data for one and the same specimen.

C. CHREE.

January 11.

The Metric System.

MR. BROOK-FOX does not state accurately, in his letter to NATURE of January 9, what passed in relation to legislation on the subject of Weights and Measures in British India in the years 1870-1871.

It is true that an Act was passed in April 1870, enabling the Governor-General in Council to require the use of the Metrical Weights and Measures by Government departments, railway or other companies, registered under the law relating to such companies, and persons exercising specified occupations or trades; but only after certain preliminary steps had been gone through.

This Act was disallowed by the then Secretary of State for India, the Duke of Argyll, who had at first authorised legislation on the subject, in the general direction given to this Act, but considered that the actual form in which the measure was passed went beyond his original instructions, as it might have been extended to the dealings of persons engaged in business or trade, and was otherwise considered to be too drastic. Objection was also taken to the inclusion of the metre as the standard of length, the original authority to legislate having been limited to the adoption of the kilogramme as the unit of weight.

Immediately on the receipt of the Secretary of State's disallowance of the Act of 1870, a new permissive Bill was prepared, extending only to the standard of weight, which was again stated to be the kilogramme, and of measures of capacity which are subsidiary to those of weight; and limited, as to the power of applying it, to the case of Government departments, municipal bodies, and railway companies.

This measure became law as Act xxxi. of 1871, and is still in force. But it has never been put into operation. This, however, was not the result of the Duke of Argyll's action, but of the change of policy that followed on Lord Mayo's most unfortunate murder. Had Lord Mayo lived, he would certainly have given effect to the Act, the importance of which he thoroughly recognised. His successor was of a different opinion, and the combination of circumstances that for the moment appeared to have rendered possible the introduction of a rational system of weights and measures into our Indian Empire, passed away, and has not yet returned.

As having had charge of the Bill of 1870, in the Council of the Governor-General, I may be permitted, in conclusion, to quote the words that I wrote twenty-five years ago with reference to the fears expressed by the Secretary of State as to the danger of precipitate action in such a matter as this.

"The history of this very Act regarding weights and measures, which has been under consideration in one form or other for not less than fourteen years, which in its last stage has taken seven or eight years in coming to maturity, and to carry out the provisions of which no measures have yet been taken, or can be taken in all probability for another year at least, seems to me to teach that the one thing perfectly certain in the future is that the progress made in giving effect to any change in the present case will be slow in a degree most painful to all who are concerned in it; and that what with active opposition, and what with indifference, this Act is far more likely to remain a dead letter on the statute book for an indefinite period, than to be brought into operation with improper haste."

January 13. RICHARD STRACHEY.

Marsupial with an Allantoic Placenta.

I HAVE just received from my friend and former pupil Mr. J. P. Hill, Demonstrator of Biology in the Sydney University, an advanced copy of the *Abstract of Proceedings* of the Linnean Society of New South Wales for November 27, in which he announces the discovery of a true allantoic and highly vascular placenta, of a discoidal and most probably deciduous type, in the Bandicoot (*Perameles obesula*).

In the second part of his magnificent "Forschungsreisen im Australien und dem malayischen Archipel," Prof. R. Semon has substantiated the discovery of apposition between the allantois and sub-zonal membrane in *Phascolarctus*, and shown that a fusion is effected. He points out that the fœtal appendages of that animal are of a type involving certain Marsupialia in a position intermediate between the Placentalia on one hand, and the Monotremata and Sauropsida on the other, and regards the earlier recognised Marsupial condition, in which the allantois is remote from the serous membrane, as secondary and associated with the loss of a respiratory function by that organ.

The condition described by Mr. Hill in *Perameles* is one of natural advance upon the Phascolarctus type, and the facts in proof of the intimacy of relationship between the Placentalia and Lower Mammalia now overlap, like those bearing on birds and reptiles—*i.e.* just as *Archæopteryx* may be regarded as an avian reptile, and the Odontorinthes as reptilian birds—so the *Phascolarctus* may be regarded as a Placental Marsupial, and those placentalia which develop a provisionally vascular yolk-sac extending to the serous-membrane as Marsupial if not Monotrematous Placentalia. Mr. Hill's discovery, coming close upon that of Woodward that the young of the Diprotodontia are found to develop at fewest five pairs of upper incisors, and of Thomas that a representative of the Epanorthidæ survives in South America, and at a time when the tooth-genesis of both Marsupialia and Placentalia is receiving exceptional attention, is as welcome as important, in assisting us to form a clearer conception of the inter-relationships between these mammals.

Mr. Hill's observation would appear to lend not a little support to the conclusion which has for years been steadily gaining ground (*cf.* NATURE, vol. xl. p. 420), that the allantoic placenta was primitively discoidal. In an accompanying letter, he informs me that he has more recently come into possession of a uterus containing an unattached blastodermic vesicle; and it is sincerely to be hoped that he will be able to furnish observations bearing directly upon the important question of the supposed primitively chorionic nature of the Mammalian placenta.

G. B. HOWES.

Royal College of Science, London, January 14.

The Origin of Plant Structures.

MAY I call attention to a serious omission, and reply to one or two points, in Mr. Barber's review of my book? He says: "Of the *inheritance* of such acquired characters there is no proof at all. We are offered instead the 'argument of coincidences' and the 'cumulative evidence of probabilities, which amounts to a moral conviction.' Clearly, before rejecting a well-established and widely applicable hypothesis, something more tangible is required" (NATURE, December 19, 1895, p. 145).

First, with regard to inheritance. Mr. Wallace also asked for some proof of this; and I reply again that nature herself supplies it; for plant structures *are* reproduced by seed every year. It is the previous question, "How have they arisen?" with which I am concerned. But I *have*, in fact, given plenty of cases: as in mv experiment with *Ononis*, Buckman's parsnip, Flahoult's with alpine plants, &c.

Moreover, the objector should state whether he means that any altered features in a plant should be reproduced by seed irrespective of the environment, or not. If a plant changes under new conditions—as ample experience shows it may—of course all its offspring will follow suit, under the same environment, irrespective of heredity; and if the conditions be maintained long enough, *then* the new features will tend to become relatively fixed, as all cultivators know. As long, however, as a natural environment is constant, no varieties are, as a rule, to be expected. Under cultivation, this rule does not hold good. Thus, *e.g. Brassica oleracea* gives rise to no varieties in nature; but there are very many fixed and hereditary races in artificial soils.

Secondly, the truth of an hypothesis or deduction cannot be more surely established than by "verification by experiment." Thus, with desert, aquatic, alpine, maritime, and other plants, I not only established the truth of my contention by *induction*, but have given the experimental verifications both of others and myself. For *this* fact Mr. Barber gives me no credit. It is in these two lines of proof, viz. by *induction* and *experiment*, that the theory of "the origin of species by means of natural selection" is wanting. It is, as Mr. Barber says, based on an "assumption," and is an *à priori* deduction that, because plants can vary indefinitely under cultivation, therefore they do so also in nature. *This has never been verified.*

Mr. Barber adds: "It is usually agreed that, from the nature of the case, a definite proof of the action of natural selection is difficult, if not impossible, in the present state of our knowledge." Is not this a most damaging admission? If the word "present" has to cover the thirty-six years since the "Origin" appeared, it would seem to be about time to abandon the theory even as a working hypothesis. The "wideness of its application" is no test of the truth of a deduction; for though natural selection may account for all organic structures, it can only do so because it is *assumed* that it can account for them. Herein it agrees precisely with the theory of special creations; which is equally assumed to be capable of accounting for every organic structure.

Mr. Barber tells me that I show a want of good taste in "narrowing" Darwin's field of observation. I much regret that anything I have written should be regarded as uncourteous; but it is Darwin himself who admits the "imputation," for he wrote: "I will give in detail all the facts which I have been able to collect," *i.e.* of "definite action" in plants, and mentions about thirty instances which he had heard of. He did not believe in definite variation being the rule in nature.

Lastly, I make no claim "to reconstruct the theory of evolution." All I have done is to take the following passage of Mr. Herbert Spencer's Essay on "The Development Hypothesis" (published in 1852, seven years before Darwin's "Origin" appeared) as my subject; and I have simply verified its profound truth in its application to plants. "The supporters of the developmental hypothesis can show . . . that any existing species—animal or vegetable—when placed under conditions different from its previous ones, immediately begins to undergo certain changes of structure fitting it for the new conditions . . . that in successive generations these changes continue until ultimately the new conditions become the natural ones. . . . They can show that throughout all organic nature there is at work a modifying influence of the kind they assign, as the causes of . . . specific differences; an influence which, though slow in its action, does in time, if the circumstances demand it, produce marked changes."

GEORGE HENSLOW.

I AM sorry I cannot agree with Prof. Henslow as to the nature of the proof required of him.

The fact that "plant structures *are* reproduced by seed every year" in nature is surely no proof of the inheritance of acquired characters!

Plants are, it is true, exceedingly plastic structures, and, as all allow, are both temporarily and permanently affected by their surroundings. This, if we needed further proof, Prof. Henslow has repeatedly demonstrated by his interesting series of facts. There seems to be danger, however, of confusing these *changes in the individual* with *changes in the race*. Prof. Henslow makes the former the prelude to the latter; and the first question to be settled is, What connection is there between these two classes of changes? In other words, Are acquired characters hereditary?

The issue is the same if we seek for the *causes of variation*. Darwinism, realising that there is a gradual adaptation of plants to altered surroundings, explains the fact by the *indirect* influence of the environment acting through natural selection. Among plants, which are stimulated to vary in all directions under change of conditions, those are preserved which vary so as to place themselves in adaptation to their new surroundings. Prof. Henslow substitutes the *direct* influence of the environment upon the *individual* plant, and asserts that the changes thus induced "become relatively fixed if the conditions are maintained long enough." Here is an assumption that the *change in the race* is the outcome of the *direct effect of the environment upon the individual*, or, again, that acquired characters are hereditary. I have carefully re-examined the cases mentioned of *Ononis*, parsnips and alpine plants, but cannot trace any proof of this assumption.

As to the *indefinite variation* of plants and animals in nature, it is difficult to conceive of doubt upon the subject. It is a common saying that "no two blades of grass are alike," although conditions could hardly be imagined more uniform than those in one and the same field. Moreover, the fact of indefinite variation has been fully proved by Prof. Wallace in his work on Darwinism. In chapter iii., on "Variability of Species in a State of Nature," the whole subject has been exhaustively dealt with, and I cannot do better than refer Prof. Henslow to that chapter, where numerous cases are given, both in animals and plants. I would especially refer to the extracts from Darwin's note-books there published for the first time.

I do not quite follow Prof. Henslow in rejecting evidence drawn from cultivated plants. Placing wild forms under cultivation is a severe change of environment, and any such change induces, of itself, a great tendency to vary. The influences which work slowly in nature are intensified; and the substitution

of artificial instead of natural selection further increases the rapidity of the results.

It must be borne in mind, too, that plants under cultivation are not necessarily grown for successive generations under the same conditions. While the change from the wild state to cultivation is as slight in some cases as it is profound in others, plants under continued cultivation are frequently subject to a succession of changes of environment as to soil, locality, water, manure, &c.; and we should therefore, according to well-known laws, expect to obtain a greatly increased number of variations in them. And these variations, as elsewhere, are coupled with a strong hereditary tendency, thus producing many new varieties.

Lastly, as regards the quotation from Mr. Herbert Spencer's essay, its terms are not at all inimical to natural selection, but apply to it equally well—a remark which, I cannot but feel, also applies to the bulk of Prof. Henslow's work. C. A. B.

A Remarkable Discharge of Lightning.

[THE following letter was sent to the Royal Society, and has been forwarded to us by the Secretary.—ED. NATURE.]

I THINK it may interest you to know that an extraordinary flash of lightning was witnessed from this place, this evening, at 7.38 p.m. It has been raining in torrents nearly all day long; the heavens seem heavy and saturated with rain, but we have had no thunder at all.

Now the undersigned were seated round a table in a room in Fife Street, and only one of us had his eyes turned in the direction of the door, which was open. Suddenly he exclaimed, "Good heavens! just look at that lightning; it's standing still!"

All of us promptly went to the door, whence we witnessed a truly extraordinary sight in the shape of three ribbons of a greenish white lightning, which hung in the sky, motionless, for what must have been fifteen to twenty seconds. It seemed to be a long way off (in a north-westerly direction), as we heard no report of thunder whatever. We put some questions to our Makalaka boy, who said that he had never seen anything like it in all his life.

There could be no mistake about it—it was as distinct as possible; and it must have lasted fifteen seconds *at least* (I should say twenty myself). I can refer you (should you desire to know more of me) to John Chumley, Esq., Manager of the Standard Bank of South Africa, Limited, 10 Clement's Lane, London, E.C.; Major W. E. Gilbert, Warleigh Lodge, Upper Tulse Hill; or John Heal, Esq., Hertford Lodge, Church End Finchley, London, N. ROB. GODLONTON.

The undersigned were witnesses of the stroke of forked lightning described in the letter to you, written by Mr. Godlonton, and consider his description accurate in every detail.

CHAS. HONEY (care of F. A. Purdon, Esq., Buluwayo).
OTTO BERTRAM (Standard Bank, Buluwayo).
ROB. GODLONTON (Secretary Matebeleland Printing and Publishing Company, Limited, Buluwayo).

December 2, 1895.

Lecture Experiments on the Nodes of a Bell.

I WAS much interested in the communication from Mr. Osborn on the above subject (see NATURE, January 9). For some years I have been in the habit of showing these nodes in the following way. An ordinary glass bell-jar, eight or ten inches high, with a moderately broad, flat, ground edge, is held with the edge upright, and fine sand scattered all over the flat edge. It is comparatively easy to excite the edge with a bow in such a way that the sand will be driven off everywhere except at the four nodes. I have never been able, however, to obtain more than four nodes in this way.

I have also employed a similar method for showing the nodes of a tuning-fork. If the fork is a moderately large one, it is held horizontally in the hand, and the upper prong is covered with sand. By bowing sharply near the middle and near the root of the prongs, two overtones can usually be obtained, the nodes of which are clearly marked by the sand.

Central School, Manchester. R. L. TAYLOR.

THE STATUS OF LONDON UNIVERSITY.

PROF. S. P. THOMPSON'S lecture to the Society of Arts on the 15th inst. will greatly assist the scheme for the reform of the University of London. The statistics brought forward by him show how hopelessly inadequate the equipment of the present University appears when compared with that of almost any other University in the world. It can hardly be believed that while Strassburg receives State aid to the extent of £44 per annum for each student, the University of London actually *pays the State* ten shillings for each student. As the lecturer remarked, a University which has no professors, no museums, no laboratories for research, whose library is practically unused and unusable, and whose sole function is to examine, cannot be called a *great* University, if, indeed, it be rightly entitled to be called a University at all. Limits of space prevent us from reprinting Prof. Thompson's paper, but we give, on the following page, a table prepared by him to exhibit the material and financial aspects of different Universities. This information, and Lord Reay's remarks upon the paper, should do much to controvert dialectic denunciations, and to show the true position of London University among the Universities of the world.

Prof. Thompson considered in succession the points upon which information is given in the different columns of his tabulated statement. He showed that not only is the educational position of the existing University entirely anomalous, but the financial position is still more extraordinary.

In closing the discussion which followed the reading of Prof. Thompson's paper, the Chairman, Lord Reay, remarked that the statistics which it included could not be too much impressed on the public mind, as an indictment against the country for leaving waste resources unparalleled in the civilised world. He was quite convinced that, if there were in any other country the treasures we had in London, both in the way of museums and libraries, and of men who were prepared to teach, it would not take ten, twelve, or twenty years to bring about the result required; but that whoever was the director of public instruction in that country would at once say to the Minister that it was his duty to lay on the table of the Legislature a Bill for the establishment of a teaching university. Among the many extraordinary symptoms which this controversy had brought to the surface, there was one of a very curious nature. Whenever they read an argument against the creation of such a university they found, either outspoken or in a latent form, this accusation: "Such a scheme will hand us over to the tender mercies of the London teachers." Now to the tender mercies of the teachers higher education was left in all the countries of Europe. He was not yet acquainted with the constitution of the University of Tokio, but he should be much surprised if they found there the slightest jealousy of leaving to the teachers the management of that which they must understand better than others. As a member of the Cowper Commission, he had been agreeably surprised to find that amongst all those on whose opinion the Commission set most store, there had been hardly a dissentient voice. In the case of every former report or scheme, those who would have had to put it in operation, and on whose labours its success depended, were in doubt, not about details, but about some leading feature; but this last scheme had been accepted not only by the teachers in London, but by the staffs of those very provincial schools whose students, they were told, in some questions had not been sufficiently considered. The best answer to the difficulty about external students was that given by Prof. Thompson when he said that learning, not teaching or examining, was the primary essential. That meant that, in a teaching university, the individuality of the teacher should be allowed its full scope, and also that each individual student should be allowed to work for the sake of learning, not for the sake of the examination. There might be as much difference between two internal students as between an internal student and an external, and in the examination the individual character of each student would be allowed for. The external students would not only have the same guarantees of a fair examination as at present, but perhaps even better; but if further guarantees were wanted, by all means let them be given. The great point was that internal students of London should, at least, have that to which they had a right—a teaching university of their own. It was nothing less than a scandal that London, with a greater population than Scotland, or than many of the countries of Europe, which had two or three universitie , should not have a university of its own.

With regard to the question of the boundary line, every one who had had any experience in such questions knew that controversy was endless, but that was a matter for the Statutory Commission, and the subjects to be included in the curriculum were also open; but agriculture was expressly included, because it was found that at most universities the science of agriculture, apart from its practical aspect, was deemed essential. He thought it was not only the duty, but the privilege of any Government of this country to at least give to the metropolis and to the empire a worthy university.

	Gross Income.	State or Municipal Subvention.	No. of Students.	Income per Student.	State, &c., Subvention per Student.	No. of Professors and Assistants.	Total Salaries of Teachers.	Total sum spent on Museums, Laboratories, Observatories, and Institutes per annum.	Total Sum spent on Library per annum. Staff.	Books.
	£	£		£	£		£	£	£	£
Paris	150,000	?	11,233	13	?	300 +	116,000	34,000	4,100	2,700
Berlin	130,000	105,000	8,652	15	12	179 + 174	34,000	73,000	1,600	950
Vienna	109,000	104,000	6,714	16	15	159 + 190	25,000	12,600	1,080	930
Oxford { Univ. Colls.	63,761 250,000	—	{ 3,200 (under-grads.) }	19 } 77 }	—	70 +	?	12,000	4,762	5,238
Cambridge { Univ. Colls.	65,550 282,000	—	{ 2,900 (under-grads.) }	22 } 97 }	—	80 +	?	9,000	4,000	2,040
Harvard ...	260,000	—	3,783	69	—	149 + 188	101,000	88,000	4,200	5,000
Leipzig	90,000	70,000	2,957	34	23	134 + 65	46,000	24,500	2,000	2,500
Edinburgh ...	88,142	29,752	2,924	30	10	90 +	48,000	19,000	1,012	1,400
London	21,000	†	2,225	9	‡	—	—	199	?	100
Cornell	105,000	7,000	1,686	62	4	77 + 80	54,000	21,300	7,600	
Padua	26,800	?	1,672	16	?	62 + 60	20,000	4,800	1,000	400
Graz	19,800	18,600	1,562	13	12	83 + 28	10,000	9,000	2,200	
Upsala	40,000	9,000	1,495	27	6	122	24,300	8,900	1,200	
Bologna... ...	30,000	?	1,457	20	?	70 + 81	20,000	9,000 (?)	680	400
Heidelberg ...	38,400	34,500	1,428	27	24	96 + 25	23,000	13,000	?	800
Tokio	70,000(?)	?	1,396	50 (?)	?	123 + 31	25,000(?)	?	?	?
Tübingen ...	45,000	43,000	1,262	36	34	69 + 15	20,000	23,000	?	700
Dublin (Trinity College) ...	70,000	—	1,124	62	—	35 +	?	?	?	
Strassburg ...	50,000	46,000	1,030	48	44	88 + 32	26,000	16,000	2,800	2,950
Greifswald ...	39,000	14,000	891	43	17	64 + 22	13,000	19,000	1,000	2,000
Zürich *	30,000	25,000(?)	822	36	30 (?)	61 + 56	9,500	3,500	[1,150]	
Leyden	62,200	?	815	76	?	50 + ?	33,000	4,680	445	780
Königsberg ...	49,000	41,000	756	66	55	70 + 32	15,000	25,000	1,300	1,370
Giessen	38,000	27,000	598	63	45	55 + 8	13,000	22,000	570	900
Baltimore ...	35,000	—	589	59	—	42 + 42	?	10,300 (?)	?	
Rostock... ...	16,000	15,000	420	38	36	42 + 3	7,900	5,500	400	1,030
St. Andrews ...	11,972	6,035	199	60	30	15 + 4	10,000	?	630	

* This does not include the Polytechnicum, which has an income of £36,000, of which £30,000 is a Subvention from Government, and which has 1,235 Students, and spends £6,500 a year on its Laboratories for Chemistry, Physics, Engineering, &c.

† Instead of receiving a Subvention, London University *pays to the State* £1,102 per annum.

‡ London University *pays to the State* a sum equivalent to 10*s*. per student.

VOTE OF CONVOCATION ON THE COWPER COMMISSION SCHEME.

ANOTHER step has been taken in the long controversy with respect to the equipment of the University of London with teaching functions. While the other bodies represented on the recent deputation to the Duke of Devonshire had passed resolutions asking the Government to introduce a Bill similar to Lord Playfair's "London University Commission Bill, 1895," but with an added clause giving a right of appeal to the Privy Council (NATURE, December 5, 1895), Convocation had not expressed any opinion either on the Bill or on the proposed appeal, owing to Lord Playfair's Bill being introduced into the House of Lords too late to allow of a resolution approving its terms to be moved at the last meeting in May. On Tuesday last, the Annual Committee recommended Convocation to adopt the following resolution: "That this House desires the early introduction into Parliament of a Bill for the reconstitution of the University similar to that introduced last year by Lord Playfair, but with an inserted clause securing to the Senate, to Convocation, and to other bodies affected, the right of appeal to the Privy Council on any of the provisions which may hereafter be settled by the Statutory Commission." This resolution was carried by 470 votes against 244, and thus for the third time Convocation, in the only legal way, has pronounced decisively in favour of the Cowper Commission scheme. The progressive rise in the majorities is not the least satisfactory feature of the struggle in Convocation—a majority of 24 in a house of 290 in January of last year rose to 122 in a house of 354 in May, and has now become 226 in a house of 714. The next step rests with the Government, but in view of the remarkable unanimity existing among the bodies affected by the scheme, and the universally favourable attitude of the metropolitan press towards it, we can be in no doubt as to what the final settlement must be.

ON A NEW KIND OF RAYS.[1]

(1) A DISCHARGE from a large induction coil is passed through a Hittorf's vacuum tube, or through a well-exhausted Crookes' or Lenard's tube. The tube is surrounded by a fairly close-fitting shield of black paper; it is then possible to see, in a completely darkened room, that paper covered on one side with barium platinocyanide lights up with brilliant fluorescence when brought into the neighbourhood of the tube, whether the painted side or the other be turned towards the tube. The fluorescence is still visible at two metres distance. It is easy to show that the origin of the fluorescence lies within the vacuum tube.

(2) It is seen, therefore, that some agent is capable of penetrating black cardboard which is quite opaque to ultra-violet light, sunlight, or arc-light. It is therefore of interest to investigate how far other bodies can be penetrated by the same agent. It is readily shown that all bodies possess this same transparency, but in very varying degrees. For example, paper is very transparent; the fluorescent screen will light up when placed behind a book of a thousand pages; printer's ink offers no marked resistance. Similarly the fluorescence shows behind two packs of cards; a single card does not visibly diminish the brilliancy of the light. So, again, a single thickness of tinfoil hardly casts a shadow on the screen; several have to be superposed to produce a marked effect. Thick blocks of wood are still transparent. Boards of pine two or three centimetres thick absorb only very little. A piece of sheet aluminium, 15 mm. thick, still allowed the X-rays (as I will call the rays, for the sake of brevity) to pass, but greatly reduced the fluorescence. Glass plates of similar thickness behave similarly; lead glass is, however, much more opaque than glass free from lead. Ebonite several centimetres thick is transparent. If the hand be held before the fluorescent screen, the shadow shows the bones darkly, with only faint outlines of the surrounding tissues.

Water and several other fluids are very transparent. Hydrogen is not markedly more permeable than air. Plates of copper, silver, lead, gold, and platinum also allow the rays to pass, but only when the metal is thin. Platinum ·2 mm. thick allows some rays to pass; silver and copper are more transparent. Lead 1·5 mm. thick is practically opaque. If a square rod of wood 20 mm. in the side be painted on one face with white lead, it casts little shadow when it is so turned that the painted face is parallel to the X-rays, but a strong shadow if the rays have to pass through the painted side. The salts of the metals, either solid or in solution, behave generally as the metals themselves.

(3) The preceding experiments lead to the conclusion that the density of the bodies is the property whose variation mainly affects their permeability. At least no other property seems so marked in this connection. But that the density alone does not determine the transparency is shown by an experiment wherein plates of similar thickness of Iceland spar, glass, aluminium, and quartz were employed as screens. Then the Iceland spar showed itself much less transparent than the other bodies, though of approximately the same density. I have not remarked any strong fluorescence of Iceland spar compared with glass (see below, No. 4).

(4) Increasing thickness increases the hindrance offered to the rays by all bodies. A picture has been impressed on a photographic plate of a number of superposed layers of tinfoil, like steps, presenting thus a regularly increasing thickness. This is to be submitted to photometric processes when a suitable instrument is available.

(5) Pieces of platinum, lead, zinc, and aluminium foil were so arranged as to produce the same weakening of the effect. The annexed table shows the relative thickness and density of the equivalent sheets of metal.

	Thickness.	Relative thickness.	Density
Platinum	·018 mm.	1	21·5
Lead	·050 ,,	3	11·3
Zinc	·100 ,,	6	7·1
Aluminium	3·500 ,,	200	2·6

From these values it is clear that in no case can we obtain the transparency of a body from the product of its density and thickness. The transparency increases much more rapidly than the product decreases.

(6) The fluorescence of barium platinocyanide is not the only noticeable action of the X-rays. It is to be observed that other bodies exhibit fluorescence, *e.g.* calcium sulphide, uranium glass, Iceland spar, rock-salt, &c.

Of special interest in this connection is the fact that photographic dry plates are sensitive to the X-rays. It is thus possible to exhibit the phenomena so as to exclude the danger of error. I have thus confirmed many observations originally made by eye observation with the fluorescent screen. Here the power of the X-rays to pass through wood or cardboard becomes useful. The photographic plate can be exposed to the action without removal of the shutter of the dark slide or other protecting case, so that the experiment need not be conducted in darkness. Manifestly, unexposed plates must not be left in their box near the vacuum tube.

It seems now questionable whether the impression on the plate is a direct effect of the X-rays, or a secondary result induced by the fluorescence of the material of the plate. Films can receive the impression as well as ordinary dry plates.

[1] By W. C. Röntgen. Translated by Arthur Stanton from the *Sitzungsberichte der Würzburger Physik-medic. Gesellschaft*, 1895.

I have not been able to show experimentally that the X-rays give rise to any calorific effects. These, however, may be assumed, for the phenomena of fluorescence show that the X-rays are capable of transformation. It is also certain that all the X-rays falling on a body do not leave it as such.

The retina of the eye is quite insensitive to these rays: the eye placed close to the apparatus sees nothing. It is clear from the experiments that this is not due to want of permeability on the part of the structures of the eye.

(7) After my experiments on the transparency of increasing thicknesses of different media, I proceeded to investigate whether the X-rays could be deflected by a prism. Investigations with water and carbon bisulphide in mica prisms of 30° showed no deviation either on the photographic or the fluorescent plate. For comparison, light rays were allowed to fall on the prism as the apparatus was set up for the experiment. They were deviated 10 mm. and 20 mm. respectively in the case of the two prisms.

With prisms of ebonite and aluminium, I have obtained images on the photographic plate, which point to a possible deviation. It is, however, uncertain, and at most would point to a refractive index 1·05. No deviation can be observed by means of the fluorescent screen. Investigations with the heavier metals have not as yet led to any result, because of their small transparency and the consequent enfeebling of the transmitted rays.

On account of the importance of the question it is desirable to try in other ways whether the X-rays are susceptible of refraction. Finely powdered bodies allow in thick layers but little of the incident light to pass through, in consequence of refraction and reflection. In the case of the X-rays, however, such layers of powder are for equal masses of substance equally transparent with the coherent solid itself. Hence we cannot conclude any regular reflection or refraction of the X-rays. The research was conducted by the aid of finely-powdered rock-salt, fine electrolytic silver powder, and zinc dust already many times employed in chemical work. In all these cases the result, whether by the fluorescent screen or the photographic method, indicated no difference in transparency between the powder and the coherent solid.

It is, hence, obvious that lenses cannot be looked upon as capable of concentrating the X-rays; in effect, both an ebonite and a glass lens of large size prove to be without action. The shadow photograph of a round rod is darker in the middle than at the edge; the image of a cylinder filled with a body more transparent than its walls exhibits the middle brighter than the edge.

(8) The preceding experiments, and others which I pass over, point to the rays being incapable of regular reflection. It is, however, well to detail an observation which at first sight seemed to lead to an opposite conclusion.

I exposed a plate, protected by a black paper sheath, to the X-rays, so that the glass side lay next to the vacuum tube. The sensitive film was partly covered with star-shaped pieces of platinum, lead, zinc, and aluminium. On the developed negative the star-shaped impression showed dark under platinum, lead, and, more markedly, under zinc; the aluminium gave no image. It seems, therefore, that these three metals can reflect the X-rays; as, however, another explanation is possible, I repeated the experiment with this only difference, that a film of thin aluminium foil was interposed between the sensitive film and the metal stars. Such an aluminium plate is opaque to ultra-violet rays, but transparent to X-rays. In the result the images appeared as before, this pointing still to the existence of reflection at metal surfaces.

If one considers this observation in connection with others, namely, on the transparency of powders, and on the state of the surface not being effective in altering the passage of the X-rays through a body, it leads to the probable conclusion that regular reflection does not exist, but that bodies behave to the X-rays as turbid media to light.

Since I have obtained no evidence of refraction at the surface of different media, it seems probable that the X-rays move with the same velocity in all bodies, and in a medium which penetrates everything, and in which the molecules of bodies are embedded. The molecules obstruct the X-rays, the more effectively as the density of the body concerned is greater.

(9) It seemed possible that the geometrical arrangement of the molecules might affect the action of a body upon the X-rays, so that, for example, Iceland spar might exhibit different phenomena according to the relation of the surface of the plate to the axis of the crystal. Experiments with quartz and Iceland spar on this point lead to a negative result.

(10) It is known that Lenard, in his investigations on kathode rays, has shown that they belong to the ether, and can pass through all bodies. Concerning the X-rays the same may be said.

In his latest work, Lenard has investigated the absorption coefficients of various bodies for the kathode rays, including air at atmospheric pressure, which gives 4·10, 3·40, 3·10 for 1 cm., according to the degree of exhaustion of the gas in discharge tube. To judge from the nature of the discharge, I have worked at about the same pressure, but occasionally at greater or smaller pressures. I find, using a Weber's photometer, that the intensity of the fluorescent light varies nearly as the inverse square of the distance between screen and discharge tube. This result is obtained from three very consistent sets of observations at distances of 100 and 200 mm. Hence air absorbs the X-rays much less than the kathode rays. This result is in complete agreement with the previously described result, that the fluorescence of the screen can be still observed at 2 metres from the vacuum tube. In general, other bodies behave like air; they are more transparent for the X-rays than for the kathode rays.

(11) A further distinction, and a noteworthy one, results from the action of a magnet. I have not succeeded in observing any deviation of the X-rays even in very strong magnetic fields.

The deviation of kathode rays by the magnet is one of their peculiar characteristics; it has been observed by Hertz and Lenard, that several kinds of kathode rays exist, which differ by their power of exciting phosphorescence, their susceptibility of absorption, and their deviation by the magnet; but a notable deviation has been observed in all cases which have yet been investigated, and I think that such deviation affords a characteristic not to be set aside lightly.

(12) As the result of many researches, it appears that the place of most brilliant phosphorescence of the walls of the discharge-tube is the chief seat whence the X-rays originate and spread in all directions; that is, the X-rays proceed from the front where the kathode rays strike the glass. If one deviates the kathode rays within the tube by means of a magnet, it is seen that the X-rays proceed from a new point, *i.e.* again from the end of the kathode rays.

Also for this reason the X-rays, which are not deflected by a magnet, cannot be regarded as kathode rays which have passed through the glass, for that passage cannot, according to Lenard, be the cause of the different deflection of the rays. Hence I conclude that the X-rays are not identical with the kathode rays, but are produced from the kathode rays at the glass surface of the tube.

(13) The rays are generated not only in glass. I have obtained them in an apparatus closed by an aluminium plate 2 mm. thick. I purpose later to investigate the behaviour of other substances.

(14) The justification of the term "rays," applied to the phenomena, lies partly in the regular shadow pictures produced by the interposition of a more or less permeable

body between the source and a photographic plate or fluorescent screen.

I have observed and photographed many such shadow pictures. Thus, I have an outline of part of a door covered with lead paint; the image was produced by placing the discharge-tube on one side of the door, and the sensitive plate on the other. I have also a shadow of the bones of the hand (Fig. 1), of a wire wound upon a bobbin, of a set of weights in a box, of a

FIG. 1.—Photograph of the bones in the fingers of a living human hand. The third finger has a ring upon it.

compass card and needle completely enclosed in a metal case (Fig. 2), of a piece of metal where the X-rays show the want of homogeneity, and of other things.

For the rectilinear propagation of the rays, I have a pin-hole photograph of the discharge apparatus covered with black paper. It is faint but unmistakable.

(15) I have sought for interference effects of the X-rays,

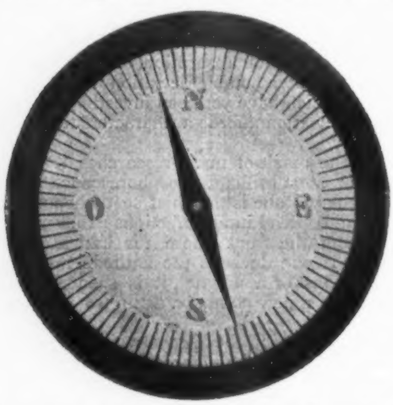

FIG. 2.—Photograph of a compass card and needle completely enclosed in a metal case.

but possibly, in consequence of their small intensity, without result.

(16) Researches to investigate whether electrostatic forces act on the X-rays are begun but not yet concluded.

(17) If one asks, what then are these X-rays; since they are not kathode rays, one might suppose, from their power of exciting fluorescence and chemical action, them to be due to ultra-violet light. In opposition to this view a weighty set of considerations presents itself. If X-rays be indeed ultra-violet light, then that light must possess the following properties.

(*a*) It is not refracted in passing from air into water, carbon bisulphide, aluminium, rock-salt, glass or zinc.

(*b*) It is incapable of regular reflection at the surfaces of the above bodies.

(*c*) It cannot be polarised by any ordinary polarising media.

(*d*) The absorption by various bodies must depend chiefly on their density.

That is to say, these ultra-violet rays must behave quite differently from the visible, infra-red, and hitherto known ultra-violet rays.

These things appear so unlikely that I have sought for another hypothesis.

A kind of relationship between the new rays and light rays appears to exist; at least the formation of shadows, fluorescence, and the production of chemical action point in this direction. Now it has been known for a long time, that besides the transverse vibrations which account for the phenomena of light, it is possible that longitudinal vibrations should exist in the ether, and, according to the view of some physicists, must exist. It is granted that their existence has not yet been made clear, and their properties are not experimentally demonstrated. Should not the new rays be ascribed to longitudinal waves in the ether?

I must confess that I have in the course of this research made myself more and more familiar with this thought, and venture to put the opinion forward, while I am quite conscious that the hypothesis advanced still requires a more solid foundation.

PROFESSOR RÖNTGEN'S DISCOVERY.

THE newspaper reports of Prof. Röntgen's experiments have, during the past few days, excited considerable interest. The discovery does not appear, however, to be entirely novel, as it was noted by Hertz that metallic films are transparent to the kathode rays from a Crookes or Hittorf tube, and in Lenard's researches, published about two years ago, it is distinctly pointed out that such rays will produce photographic impressions. Indeed, Lenard, employing a tube with an aluminium window, through which the kathode rays passed out with comparative ease, obtained photographic shadow images almost identical with those of Röntgen, through pieces of cardboard and aluminium interposed between the window and the photographic plate.

Prof. Röntgen has, however, shown that this aluminium window is unnecessary, as some portion of the kathode radiations that are photographically active will pass through the glass walls of the tube. Further, he has extended the results obtained by Lenard in a manner that has impressed the popular imagination, while, perhaps most important of all, he has discovered the exceedingly curious fact that bone is so much less transparent to these radiations than flesh and muscle, that if a living human hand be interposed between a Crookes tube and a photographic plate, a shadow photograph can be obtained which shows all the outlines and joints of the bones most distinctly.

Working upon the lines indicated in the telegrams from Vienna, recently published in the daily papers, I have, with the assistance of Mr. J. C. M. Stanton, repeated many of Prof. Röntgen's experiments with entire success. According to one of our first experiments, an ordinary gelatinous bromide dry photographic plate was placed in an ordinary camera back. The wooden shutter of the back was kept closed, and upon it were placed miscellaneous articles such as coins, pieces of wood, carbon, ebonite, vulcanised fibre, aluminium, &c., all being quite opaque to ordinary light. Above was supported a

Crookes tube, which was excited for some minutes. On development, shadows of all the articles placed on the slide were clearly visible, some being more opaque than others. Further experiments were tried with thin plates of aluminium or of black vulcanised fibre interposed between the objects to be photographed and the sensitive surface, this thin plate being used in place of the wood of the camera back. In this manner sharper shadow pictures were obtained. While most thick metal sheets appear to be entirely opaque to the radiations, aluminium appears to be relatively transparent. Ebonite, vulcanised fibre, carbon, wood, cardboard, leather and slate are all very transparent, while, on the other hand, glass is exceedingly opaque. Thin metal foils are moderately opaque, but not altogether so.

As tending to the view that the radiations are more akin to ultra-violet than to infra-red light, it may be mentioned that a solution of alum in water is distinctly more transparent to them than a solution of iodine in bisulphide of carbon.

So far as our own experiments go, it appears that, at any rate without very long exposures, a sufficiently active excitation of the Crookes tube is not obtained by direct connection to an ordinary Rhumkorff induction coil, even of a large size. So-called high frequency currents, however, appear to give good results, and our own experiments have been made with the tube excited by current obtained from the secondary circuit of a Tesla oil coil, through the primary of which were continuously discharged twelve half-gallon Leyden jars, charged by an alternating current of about 20,000 volts pressure, produced by a transformer with a spark-gap across its high-pressure terminals.

For obtaining shadow photographs of inanimate objects, and for testing the relative transparency of different substances, the particular form of Crookes tube employed does not appear to greatly signify, though some forms are, we find, better than others. When, however, the human hand is to be photographed, and it is important to obtain sharp shadows of the bones, the particular form of tube used and its position relative to the hand and sensitive plate appear to be of great importance. So far, owing to the frequent destruction of the tubes, due to overheating of the terminals, we have not been able to ascertain exactly the best form and arrangement for this purpose, except that it appears desirable that the electrodes in the tube should consist of flat and not curved plates, and that these plates should be of small dimensions.

The accompanying photograph of a living human hand (Fig. 1) was exposed for twenty minutes through an aluminium sheet ·0075 in thickness, the Crookes tube, which was one of the kind containing some white phosphorescent material (probably sulphide of barium), being held vertically upside down, with its lowest point about two inches above the centre of the hand.

FIG. 1.—Photograph of a living human hand.

By substituting a thin sheet of black vulcanised fibre for the aluminium plate, we have since been able to reduce the exposure required to four minutes. Indeed with the aluminium plate, the twenty minutes' exposure appears to have been longer than was necessary. Further, having regard to the great opacity of glass, it seems probable that where ordinary Crookes tubes are employed, a large proportion of the active radiations must be absorbed by the glass of the tube itself. If this is so, by the employment of a tube partly constructed of aluminium, as used by Lenard, the necessary length of exposure could be much reduced.

A. A. C. SWINTON.

NOTES.

AT their scientific meeting on March 3, the Zoological Society propose to discuss the much-vexed question of zoological nomenclature. This subject will be introduced by Mr. Sclater, the Secretary of the Society, who will read a paper on the "Rules for naming Animals," lately adopted by the German

Zoological Society, and point out the divergences between them and what is called the Stricklandian Code of Nomenclature, recognised by the British Association, and usually followed in this country.

THE question of technical and commercial attachés to British Embassies was referred to by the Right Hon. George Curzon, M.P., Under Secretary for Foreign Affairs, in the course of some remarks to the Wolverhampton Chamber of Commerce last week. He acknowledged that it is desirable, in many of those countries where our commercial connection is large, to have representatives whose attention is exclusively directed to our commercial interests. He also remarked that it appeared to his Department that the existing arrangement of the areas of our commercial attachés in Europe, of which there are three, is unscientific and imperfect. A technical attaché should be appointed in Germany, and our forces in other directions ought to be increased. But while he agreed that attachés with technical knowledge could be of great assistance in the development of British trade, and hoped that the Foreign Office would soon be able to do something to extend the Consular service in the desired direction, he thought that Chambers of Commerce should do more than they seem to do at present in furtherance of their own trades, by sending out commissions to distant parts to ascertain on the spot what are the particular points with which they have to contend, and the lines of possible development. This is typical of the kind of advice usually given by the British Government. It amounts to an acknowledgment of defects, but leaves private enterprise to supply the remedy. In this respect our Government differs in policy from those of France and Germany—two of our chief competitors in trade—for in those countries the State takes an active interest in the development of industries; and the knowledge which makes for such advancement, as well as the men who are competent to form an opinion upon industrial processes, are fully utilised. The Foreign Office exists for the promotion and fostering of British trade, as well as for the handling of our relations with foreign Powers. It certainly does something to supply the commercial world with trade statistics received through its Consuls, but it could do much more for the real advancement of industry if it were advised and guided in technical matters by scientific opinion. The collection of facts useful in the improvement of industrial methods and processes should form a very important part of the work carried on at our Consulates. To leave this valuable work to men sent out by traders, is to court defeat by the countries which recognise the value of attachés possessing technical and scientific knowledge. We agree that our Chambers of Commerce might adopt with advantage the forward policy of some of the French Chambers of Commerce, but, at the same time, we hold that the comparison of the Chambers of the two nations does not show British traders to extreme disadvantage as regards enterprise, whereas the work of the British Government in the same direction is not comparable with that of other nations. As in trade, so in science and education, individuals have done their share in the work of advancement; it is the Government which lags behind.

As an example of what private associations do to discover in what way industrial practices and processes on the continent differ from those in Great Britain, we may mention the delegation recently organised by the British Iron Trade Association for the purpose of inquiring into the conditions of competition in the iron and steel industries of the continent. The report of the delegates has just been finally adopted by the Association, and one or two points in it furnish instructive reading. It is admitted that both Belgium and Germany are well to the front in regard to mechanical appliances. The general arrangement of the blast furnace plants was modern and excellent, and they appeared to be worked to secure a large output with a minimum of capital and labour expenditure. In the finished iron and steel works visited, the practice adopted was excellent, and in some respects in advance of England. But of more especial interest to us is the fact that attention is called to the greater importance attached in Germany than in England to technical education, and the facilities given for its acquisition. This is regarded as one of the reasons why Germany has been able to quickly acquire, and make rapid advances in, iron and steel manufacture. On account of the attention given to scientific and technical education, the masters, as a rule, thoroughly understand the principles of their industrial processes, and are therefore able to economise in matters of detail; and moreover, they recognise the value of scientific advice and guidance upon all questions of improved methods.

THE appointment of Scientific Adviser to the Trinity House, which has been in abeyance since the resignation of Dr. Tyndall, and which was formerly held by Prof. Faraday, has been revived, and has been accepted by Lord Rayleigh.

THE engineer to the scheme for bringing sea-water from Lancing to London has prepared, for the information of Parliament, an estimate of the entire cost of this scheme. He states that the works contained in the Bill can be carried out for £450,000, which includes the acquisition of all the necessary lands for the distribution of sea-water throughout London.

A REUTER telegram from Christiania announces that Lieut. Ewind Astrup, the well-known Polar explorer, who took part in both Lieut. Peary's Greenland expeditions, was found dead on Tuesday in the Lille-Elvedal. Lieut. Astrup left Jerkin, at the foot of Sneehätta, in the Hovre Fjeld, shortly before Christmas, but since then nothing had been heard of him. A few days ago twenty-five men on snow-shoes started out to search for the explorer, with the result that they found his remains in the valley.

THE Committee of the Œuvre de la Tuberculose, founded by the late Prof. Verneuil, have (says the *British Medical Journal*) elected Prof. Bouchard to succeed that distinguished surgeon in the office of President. Prof. Lannelongue was at the same time elected Vice-President, and Dr. L. Henri Petit, General Secretary. The next Congress on Tuberculosis, which had been fixed for July 1896, has been postponed to 1897.

WE are informed that the Geological Society will this year award the following medals and funds:—The Wollaston medal to Prof. E. Suess, the Murchison medal to Mr. T. Mellard Reade, the Lyell medal to Mr. A. Smith Woodward, the proceeds of the Wollaston fund and part of the Barlow-Jameson fund to Mr. Alfred Harker, the proceeds of the Murchison fund to Mr. Philip Lake, the proceeds of the Lyell fund to Dr. W. F. Hume and Mr. W. C. Andrews, the proceeds of the Barlow-Jameson fund to Mr. Joseph Wright and Mr. John Storrie.

AN International Exhibition is to be held at Brussels in 1897. The exhibition will comprise fourteen sections: 1. Fine arts. 2. Social economy. 3. Hygiene. 4. Life saving. 5. Industrial and decorative arts. 6. Lighting and heating. 7. Electricity—traction. 8. Military art. 9. Industrial manufactures—materials, methods, and products. 10. Sporting material. 11. Exercises—popular games. 12. Temporary competitions in agriculture and horticulture. 13. Practical teaching, and the industries and handiwork of women. 14. Trade. Colonies.

A PRELIMINARY account of the Florentine earthquake of last May 18, the strongest felt in the district during the present century, has been written by Dr. M. Baratta. The chief damage

to buildings occurred within a nearly circular area about 35 km. in diameter, the centre of which is close to Florence. The disturbed area contains about 27,000 sq. km. Under the form of microseismic movements, the earthquake was recorded at several Italian observatories, and also at Grenoble (France), which is 460 km. from Florence, the mean velocity to the latter place being 1·75 km. per second.

ACCORDING to a Reuter telegram from Christiania, the estate of Hafslund, near the great waterfall known as the Sarpsfos, between there and Göteborg, has been acquired by a syndicate, consisting chiefly of German and American capitalists, for the sum of 800,000 kroner. The purchasers intend to form a large company with a capital of three to five million kroner, in order to utilise the water power of the falls for electrical power, and establishing aluminium works on the same principle as planned at the Falls of Foyers in Scotland. The Sarpsfos is one of the finest falls in south-eastern Norway, being 74 feet in height and 116 feet in width.

THE ancestral history of the horse, long familiar as a most striking instance of the possibility of tracing a pedigree by the aid of palæontology, is now being worked out still more minutely. In a paper just published (*Bull. Amer. Mus. Nat. Hist.*, December 23, 1895) on the Perissodactyls of the White River beds (Oligocene and Lower Miocene), Messrs. Osborn and Wortman announce the discovery of so complete a series of intermediate forms between *Mesohippus Bairdi* and *Anchitherium præstans*, that a strict definition of generic and specific names becomes impossible. These two and the intermediate types "form a closely-connected phylogenetic series of animals, slowly specialising and constantly increasing in size. So far as we know," the authors add, "there is not a single character missing in the structural chain."

AMONG the many points of interest which they possess, the Ratite birds of the southern hemisphere have presented no feature more curious and more inexplicable than the opercular fold which covers the transitory gill-slits in the neck of the embryo. The fold was first discovered five years ago by Prof. T. J. Parker, who described its occurrence in embryos of the New Zealand *Apteryx*; but its recent discovery by Prof. Nassonow in embryos of the ostrich also (*Zool. Anzeiger*, No. 492) shows that it is a feature which probably characterises the development of all the Ratite birds. A branchial operculum has not, however, been observed in the early stages either of reptiles or of Carinate birds, and the retention of so obviously amphibian a character in Ratite birds alone among Sauropsida provides a puzzle for ornithologists and embryologists alike.

IT is interesting to note, with reference to the observation of of the meteor seen at Cahirciveen on the 6th inst. (NATURE, p. 253), notice of which was sent to us by Mr. R. H. Scott, that according to a letter received at the Meteorological Office from Dr. A. Riggenbach, of Basle, a similar phenomenon was seen at Hochwald, about seven miles to the south of the former place, at 6 p.m. on the same evening. The observer noticed over the mountains of Nunningen what appeared to be a star of unusual magnitude. After looking at it for some time he was surprised to find it moving and leaving a long trail after it. The light increased in splendour, being first of a yellowish colour, which, upon the bursting of the meteor into three or four pieces, assumed a bluish-green colour. The movement continued in a south-westerly direction for about $1\frac{1}{2}$ seconds, when the phenomenon disappeared below the horizon.

THE history of the introduction of the first hermetically-sealed thermometer into France is contained in *Cosmos* of the 4th inst., with extracts from the correspondence of Pierre des Noyers, Secretary of the Queen of Poland. In 1657 he sent a description of a Florentine thermometer to M. Boulliau, of Paris, with a drawing of it, which is reproduced in *Cosmos*, and in 1658 a specimen of the instrument was sent to Paris, and was apparently used by M. Boulliau on June 25 of that year. This thermometer was graduated by means of small black enamelled knobs on the outside of the tube, and was subsequently improved by the Accademia del Cimento. A full description of the instrument is contained in Prof. Hellmann's "Anfänge der meteorologischen Beobachtungen und Instrumente" in *Himmel und Erde*, vol. ii. p. 175 (see NATURE, vol. xlii. p. 207). We are indebted to M. G. Maze for the publication of extracts from the interesting original documents which are contained in the National Library in Paris.

AN account of some ingenious experiments on wind pressure is reported in *Engineering* of December 27 last, made by Messrs. H. C. Vogt and I. Irminger, of Copenhagen, to prove that "the greater part of the lifting power of an aeroplane, or the thrust of a close-hauled sail, was to be attributed to the rarefaction on the leeward side of the same, and not to the direct pressure." The experiments seem to show that even when a pressure-plate was perpendicular to the wind, the direct pressure was equal to only about one-third of the vacuum behind, and that when the vane was placed parallel to the wind, a negative pressure was indicated. For the purpose of the experiments, a current of air was produced by inserting in the side of a high chimney connected with the Gas Works one end of a rectangular conduit, the interior sectional area of which was $4\frac{1}{2} \times 9$ inches, and the length 40 inches. In this conduit was placed a hollow vane which reached from side to side, but the width of which was only $1\frac{5}{8}$ inches. The interior of the hollow vane was connected through its axis with a water-gauge, and the arrangement was such that the vane could be turned to any desired angle. Three holes were bored in both faces of the vane, but only one of these holes was used at a time, the others being stopped when not in use. We think that the results arrived at should be accepted with caution. The second leg of the pressure-gauge was connected with the inside of the conduit by an opening just above the vane, and there seems to be little doubt that the disturbance of the air current must, in such a small passage, have been considerable, and would probably affect the pressure upon the hole connected with the second leg of the gauge. This may possibly account for some of the rather improbable results obtained.

The Scalpel—a monthly journal of medicine and surgery—has just made its bow to a medical public. The new journal is edited by Dr. T. M. Dolan, and it will be conducted upon much the same lines as the late *Provincial Medical Journal*.

THE January *Journal* of the Sanitary Institute (vol xvi. part iv.) contains a paper on infectious diseases and methods of disinfection, by Dr. E. Seaton; and among the articles is one on the Hermite process for sanitation of towns, by Mr. E. J. Paterson.

THE careers of few men inspire such international admiration as the life of Huxley. His sterling qualities stand out so boldly that they command the attention of the entire intellectual world. The latest biographical notice that we have seen has appeared in the last two numbers of the *Revue Scientifique*, the author being Dr. H. de Varigny. We commend Dr. Varigny's appreciative notice to the attention of those who would be interested to learn of the high regard in which Huxley's works are held among men of science in France.

WE have received from Prof. G. Klebs, of Basel, an interesting paper entitled *Ueber einige Probleme der Physiologie der Fortpflanzung*. He deals with all the most recent observations on the phenomena of non-sexual propagation and of sexual reproduction, especially in the vegetable kingdom, and sums up

in favour of the view that the non-sexual was the original universal mode of multiplication. He supports this view largely by the phenomena of parthenogenesis, and by the fact that it is possible, by preventing the conjugation of the two gametes in *Spirogyra*, to cause each of them to develop into a "spore" fully capable of germination.

THE additions to the Zoological Society's Gardens during the past week include a Mozambique Monkey (*Cercopithecus pygerythrus*, ♂) from East Africa, presented by Miss Louisa Hutt; a Pig-tailed Monkey (*Macacus nemestrinus*, ♂) from Java, presented by Mr. W. Englehardt; a Puff Adder (*Vipera arietans*) from South Africa, presented by Mr. J. E. Matcham; two Golden-crowned Conures (*Conurus aureus*) from South-east Brazil, deposited; two Black-headed Caiques (*Caica melanocephala*) from Guiana, a Green-cheeked Amazon (*Chrysotis viridigenalis*), from Columbia, purchased.

OUR ASTRONOMICAL COLUMN.

A NEW AUSTRALIAN OBSERVATORY.—The site for the new observatory, which the Government of West Australia are about to erect, has been selected at Perth. The cost of the buildings and instruments is estimated at about £5000. The latter will have such improvements on ordinary construction as have been suggested by experience with the instruments used at the Adelaide Observatory. The equipment will include an equatorial having an object-glass of 8 inches aperture. This instrument will be furnished with a spectroscope, and with photographic appliances. There will also be a transit circle 6 inches in diameter. The post of Astronomer has been filled by the appointment of Mr. W. Ernest Cooke, the Assistant Astronomer of South Australia. Mr. Cooke is an Australian by birth, and has been trained under Sir Charles Todd in observatory work.

THE SUN'S PATH IN SPACE.—The question as to a possible orbital movement of the solar system is revived by Mr. G. C. Bompas in the current number of the *Observatory*. Determinations of the position of the apex of the sun's way have been variously based on groupings of selected stars according to number, magnitude, or proper motion, the latter being now acknowledged the best test of distance. When arranged according to the distances of the stars on which they are based, the results seem to indicate that the position deduced from the nearer stars moves south-west along the plane of the Milky Way, as compared with the apex derived from stars supposed to be more distant. If the sun's path were rectilinear and the stars at rest, the "radiant point" would be the same for near and distant stars, but if curvilinear it would be shifted in accordance with the varying direction of the tangent, and the displacement would be in proportion to the apparent velocity. From existing data, Mr. Bompas considers it probable that the sun moves in a retrograde orbit from east to west in a plane inclined a few degrees to the Milky Way, and the conclusion is independent of any further consideration as to the presence or absence of a systematic movement of the stars round a common centre. "The relation of the solar system to the galaxy would thus resemble that of Uranus and his moons to the ecliptic, the solar system being inclined about 70° to the plane of the galaxy and the planets revolving round the sun in a reverse direction to the orbit attributed to the sun."

EQUATORIAL VELOCITY OF JUPITER.—A spectroscopic determination of the equatorial velocity of rotation of Jupiter has been made by Dr. Belopolsky, the resulting value being 11·4 kilometres per second (*Ast. Nach.* 3326). Taking the rotation period to be 9h. 50m., the calculated velocity varies from 12·1 to 12·8 kilometres per second, according to the angular diameter which is accepted. Although the difference between the observed and computed velocity is possibly within the limits of error in measurement, it is not considered improbable that the apparent discrepancy may have its origin in the increase of the angular diameter of the planet due to refraction in its atmosphere, and the consequent apparent increase of the calculated linear velocity. A similar result has been observed in the case of Saturn, the measured velocity being smaller than that which the known rotation period and angular diameter require.

PERRINE'S COMET.—The comet discovered by Mr. Perrine on November 16, 1895, was well observed in the northern hemisphere up to December 7, when it was seen at Greenwich, and stated to be very bright with a conspicuous tail. It was thought that it might possibly be subsequently observed in daylight in the southern hemisphere, but no observations have been published so far as we are aware. Towards the end of January it will be sufficiently removed from the sun to be seen as a morning comet, but with greatly diminished brightness. The following ephemeris, for Berlin midnight, is by Dr. E. Lamp:—

	R.A. h. m. s.	Decl. ° ′	Brightness.
Jan. 24	19 37 27	−7 3·4	0·43
28	39 13	5 51·2	0·35
Feb. 1	40 50	4 41·6	0·29
5	42 17	3 33·7	0·25
9	43 34	2 27·0	0·21
13	44 40	1 21·0	0·18
17	45 33	−0 15·5	0·16
21	46 13	+0 49·8	0·15
25	46 38	1 55·1	0·13
29	46 48	+3 0·7	0·12

The unit of brightness is that on November 18.

OYSTERS AND TYPHOID.

THE fourth meeting of the tenth session of the Liverpool Biological Society was held on January 10, at University College, Liverpool. During the evening an interesting report on "Green Oysters, and the connection between Oysters and Disease" was presented by Prof. Herdman, who explained that a year ago he and Prof. Boyce commenced to investigate the conditions under which oysters lived healthily. Among other matters they directed their particular attention to the possibility of the oyster being infected by sewage in sea-water with the typhoid organism, and of so transmitting the disease to the consumer. At the meeting of the British Association at Ipswich, last September, they communicated the results they had obtained up to that point, and then they were appointed (with the addition of Prof. Sherrington and Mr. G. C. Bourne) as a committee to investigate the matter further. At present they were really in the midst of their observations; and the present communication could only be regarded in the nature of an interim report, as their conclusions would not be drawn up for publication until the meeting of the British Association in September next. In the meantime a most alarming and widespread scare, following upon incidents connected with a ball at Stirling, on October 1, had arisen, it being assumed that there was some connection between oysters and an outbreak of typhoid. This had considerably affected the important oyster trade of the country, and had probably thrown a great deal of quite undeserved suspicion upon perfectly wholesome oysters. Under these circumstances they had felt it their duty to take an early opportunity of stating their results and impressions as they stood at present. Their work, so far as it had gone, was of a reassuring character, and demanded from the public at the very least a suspension of judgment, whilst it indicated that the adoption of some simple sanitary precautions would, if properly carried into effect, go far to remove suspicion from the oyster. Prof. Herdman then proceeded to describe, with the aid of lantern slides, specimens and microscopic preparations, the different descriptions of oysters which are supplied in North-west Europe, and the methods of treatment they are subjected to prior to being placed on the market. He dealt with the cultivation of the French green oyster, and discussed the cause of the green colour, both in that oyster and in Americans relaid on the Lancashire coast. He pointed out that the most important precaution to take in oyster culture was to choose perfectly healthy grounds for the fattening process, it being necessary, in the first place, to ascertain that the purity from sewage of the water was beyond question. Further it was advisable to submit the oysters for a short time to disgorging basins or tanks, a method which was adopted with success by the French, before sending them to the market. Prof. Boyce then followed with an account of the experiments on the infection of oysters with typhoid, and showed, by means of tables, the rate at which the typhoid bacillus disappeared in sea-water. There was no evidence of increase in numbers of the bacillus when grown in sea-waters, either when incubated or at ordinary temperatures.

THE SMITHSONIAN INSTITUTION.[1]

II.

THE SYSTEM OF ADMINISTRATION.

THE Smithsonian Institution was formally established by the Act of Congress approved August 10, 1846. As defined in the fundamental Act the "Establishment," a body which is in fact "the Institution," is composed of the President of the United States, who is presiding officer *ex officio*, the Vice-President, the Chief Justice, and the members of the Cabinet; and the body thus constituted is made responsible for the duty of "the increase and diffusion of knowledge among men."

In addition to the "Establishment," the Act provides for a "Board of Regents," by whom the business of the Institution is administered, composed of the Vice-President of the United States, the Chief Justice of the Supreme Courts, three members of the Senate, three members of the House of Representatives, and six citizens, no two of whom may be from the same State, though two must be residents of the District of Columbia.

The presiding officer of the Regents is the Chancellor, whom they may elect from their own number. This position has, however, customarily been held by the Vice-President or by the Chief Justice. The executive officer is the Secretary of the Institution, who is elected by the Regents, and is also the Secretary of that Board. The duties and responsibilities of the Secretary are, as has already been explained, such as in other institutions usually belong to the office of Director. He presents to the Regents an annual report upon the operations, expenditures, and condition of the establishment, which is transmitted by the Board to Congress for publication. By special Act of Congress of 1884 an acting-Secretary is provided, in case of the absence or disability of the Secretary, the designation being left with the Chancellor of the Institution. There is at present but one assistant-Secretary, who is in charge of the National Museum.

The annual meeting of the Regents is held in January; their executive committee of three members meet quarterly.

The building occupied by the Institution, and bearing its name, is an ornate structure of Seneca brown stone, occupying a prominent position in the "Mall," which extends from the Capitol to the Washington Monument, in the square known as the Smithsonian Park. This edifice, planned by James Renwick, was begun in 1847 and completed in 1855. Features from different periods of Romanesque styles are combined in its architecture; but its exterior, owing chiefly to the irregular skyline, is very picturesque and pleasing.

The eastern wing of the building, for so many years the hospitable home of Prof. Henry, has been reconstructed internally, and the offices of the Institution are all established within its walls. The remainder of the building is occupied by the laboratories and exhibition halls of the National Museum.

Another building of brick, 325 feet square, was built east of the Smithsonian in 1881, for the reception of a portion of the Museum collections.

THE OBJECTS OF THE INSTITUTION.

The objects of the Institution, as defined by Henry, are, first, to increase knowledge by original investigations and study either in science or literature; and, second, to diffuse knowledge, not only through the United States, but everywhere, especially by promoting an interchange of thought among those prominent in all nations. No restriction is made in favour of any one branch of knowledge.

The leading features of the plan of Prof. Henry were, in his own words, "to assist men of science in making original researches, to publish them in a series of volumes, and to give a copy of them to every first-class library on the face of the earth." There are not many scientific investigators in the United States to whom a helping hand has not at some time been extended by the Institution, and the hand has often reached across the Atlantic. Books, apparatus, and laboratory accommodation have been supplied to thousands, and each year a certain number of money grants have been made. Not less important has been the personal encouragement afforded, and the advice given in the tens of thousands of letters of information written in response to inquiries.

It is not, as some persons suppose, a teaching institution, nor does it receive students. It constantly aids, however, in the improvement of the educational system of the country.[1]

An important feature in the educational work of the Institution has been its participation in the various International Expositions. It was represented at Philadelphia in 1876; Berlin, 1880; London, 1883; New Orleans, 1885; Cincinnati, 1889; Madrid, 1892; Chicago, 1893; Atlanta, 1895; and has received many medals and diplomas of commendatory nature upon these occasions.

THE PUBLICATIONS.

The publications are numerous, and include many important and authoritative works. There is no restriction as to subject, and they consist of memoirs upon archæology, ethnology, botany, zoology, geology, palæontology, meteorology, magnetics, physics, physiology, and philology, and many other branches of investigation.

These books are practically given away, for although there is a provision for their sale at cost price, only a few hundred dollars worth are sold each year. They are regularly distributed to about 4000 institutions in all parts of the world, and are supplied also to numerous private investigators. There are several series, the aspect of which must be familiar to every observing person who has ever spent a day among the shelves in any American library of respectable standing.

(1) The Annual Report of the Regents to Congress, of which the forty-ninth, that for 1894, is now in press. Since 1884 the report of the Museum has been printed in a separate volume (Part II.).[2]

(2) The Smithsonian Contributions to Knowledge, thirty-two volumes in quarto, containing over 7000 pages and many fine plates.[3]

(3) The Smithsonian Miscellaneous Collections, in thirty-five octavo volumes, aggregating about 22,000 pages.[4]

(4) The Bulletins of the National Museum, fifty in number, beginning in 1875.[5]

(5) The Proceedings of the National Museum, including already 1100 separate papers, embraced in seventeen annual volumes, beginning in 1878.[6]

(6) The Annual Reports of the Bureau of Ethnology, beginning in 1879, and forming a series of twelve illustrated volumes in royal octavo.[7]

(7) The Bulletin of the Bureau of Ethnology, of which twenty-six numbers have appeared.[8]

The value of the books distributed since the Institution was opened has been nearly 1,000,000 dollars, or nearly twice the original bequest of Smithson.[9] Many of the publications in each of these series are now out of print.

THE LIBRARY.

One of the most important features of the Institution is the library which has grown up under its fostering care. For nearly fifty years its publications have been distributed throughout the world to almost every scientific and literary establishment of good repute. In return for these, and by purchase, it has received the great collection of books which forms its library, and which is one of the richest in the world in the publications of learned societies, and therefore of inestimable value, containing as it does the record of actual progress in all that pertains to the mental and physical development of the human family, and affording the means of tracing the history of every branch of

[1] By Dr. G. Brown Goode. (Continued from page 261.)

[1] The Institution supports a table at the International Zoological Station in Naples for the benefit of naturalists. There is an assembly hall in the Museum building, in which meetings of scientific bodies of national scope are held. Here the National Academy of Sciences holds its annual meeting every April, and the American Historical Association (which is by law affiliated with the Institution) its December meeting. Here also each year a course of popular scientific lectures is delivered under the direction of the scientific societies of Washington.

[2] "Public Documents" printed by order of Congress, and distributed in large editions.

[3] Published at the cost of the Smithsonian Fund, and not "Public Documents."

[4] *Ibid.*

[5] Published in a limited edition from a special appropriation, and not "Congressional Documents."

[6] *Ibid.*

[7] "Public Documents" printed by order of Congress, and distributed in large editions.

[8] The Bureau also supervises a series of quarto volumes, bearing the title "Contributions to North American Ethnology," begun in 1877 by the U.S. Geographical and Geological Survey, of which nine have been issued.

[9] This estimate is based upon the prices which are charged for the books by second-hand dealers, as shown in their sale catalogue.

positive science since the days of the revival of letters until the present time.

This library was, in 1865, deposited at the Capitol, as a portion of the Congressional Library.

The Smithsonian Collection, which includes more than three hundred thousand volumes and parts of volumes, constituting perhaps one-fourth of the National Library, is to be installed in a special stack-room of its own upon the main floor of the new Library Building, with a commodious reading-room adjacent for the use of special students. The rapidity with which it is increasing is indicated by the fact that in 1894, 37,952 titles were added.[1]

The Institution has probably done more towards building up a great library in Washington than would have been possible had all its income been devoted strictly to library work, as was at one time seriously proposed.

THE NATIONAL MUSEUM.

The Smithsonian Institution is the custodian of the National Museum, which is the only lawful place of deposit of "all objects of art and of foreign and curious research, and all objects of natural history, plants, and geological and mineralogical specimens, belonging to the United States." The nucleus of the collections consists of the specimens brought home by the Wilkes and other exploring expeditions, but for many years the Museum was supported entirely at the expense of the Smithson Fund, and a considerable portion of the collections is the property of the Institution. Since 1881, the entire expenses of administration have been met by Congressional appropriations. The appropriations from 1858 to 1880 were only sufficient to meet these expenses in part.

FIG. 6.—The New Museum Building.

Prof. Huxley defines a museum as "a consultative library of objects." The National Museum is such a consultative library, and it is a great deal more. It is an agency for the instruction of the people of the whole country, and it keeps in mind the needs of persons whose lives are not occupied in the study of science as well as those of the professional investigator and teacher.

Its benefits are extended without cost or reserve to hundreds of thousands of visitors from all parts of the United States who pass through its doors each year, as is shown in the following table:

Number of Visitors since 1881.

Year.	New Building.	Old Building.	Total.
1881	150,000	100,000	250,000
1882	167,455	152,744	320,199
1883	202,188	104,823	307,011
1884 (half-year)	97,661	41,565	139,226
1884-85	*205,026	102,093	307,119
1885-86	174,225	88,960	263,185
1886-87	216,562	98,552	315,114
1887-88	249,665	102,863	352,528
1888-89	*374,843	149,618	524,461
1889-90	274,324	120,894	395,218
1890-91	286,426	111,669	398,095
1891-92	269,825	114,817	384,642
1892-93	*319,930	174,188	494,118
1893-94	195,748	103,910	299,658
1894-95	196,375	109,847	306,222
	3,380,253	1,676,543	5,056,796

* Years of Presidential inaugurations.

And also through the distribution of the duplicate specimens in the Museum, which are made up into sets, accurately named, and given to public institutions in all parts of the country.

The history of the Museum is divided into three periods: First, that from the foundation of the Smithsonian Institution to 1857, during which time specimens were collected purely and solely to serve as materials for research, no special effort having been made to publicly exhibit them or to utilise them, except as a foundation for scientific description and theory. Second, the period from 1857, when the Institution assumed the custody of the "National Cabinet of Curiosities," to 1876. During this period the Museum became a place of deposit for scientific material which had already been studied; this material, so far as practicable, being exhibited to the public, and thus made to serve an educational purpose. Third, the present period, beginning in the year 1876, during which the Museum has entered upon a career of active work in gathering collections and exhibiting them on account of their educational value.

During the first period, the main object of the Museum was scientific research; in the second, the establishment became a museum of record as well as of research; while in the third period there is growing up also the idea of public education.

The three ideas, *Record*, *Research* and *Education*, co-operative and mutually helpful as they are, are essential to the development of every great museum. The National Museum endeavours to promote them all.

It is a *Museum of Record*, in which are preserved the material foundations of an enormous amount of scientific knowledge—the types of numerous past investigations. This is especially the case with those materials that have served as a foundation for the reports upon the resources of the United States.

It is a *Museum of Research*, which aims to make its contents serve in the highest degree as a stimulus to inquiry and a foundation for scientific investigation. Research is necessary in order to identify and group the objects in the most philosophical and instructive relations, and its officers are therefore selected for their ability as investigators, as well as their trustworthiness as custodians.

It is an *Educational Museum*, through its policy of illustrating by specimens every kind of natural object and every manifestation of human thought and activity, of displaying descriptive labels adapted to the popular mind, and of distributing its publications and its named series of duplicates.

The collections are installed, in part, in the Smithsonian

[1] The working libraries of the National Museum and the Bureau of Ethnology are distinct from the general Smithsonian Library, and are separately administered. All of these are placed at the service of advanced students and specialists.

building and, in part, in the large building adjacent, covering three and a half acres of ground, which was erected in 1881 to afford temporary accommodation for the overflow until such time as an adequate new building could be constructed.

The number of specimens in the various departments of the Museum, in 1894, is shown in the following table:

STATISTICS OF THE NATIONAL COLLECTIONS.

Arts and Industries.	
Historical collections, coins, medals, &c.	29,998
Musical instruments	1,219
Modern pottery, porcelain, bronzes, &c.	3,583
Graphic arts	1,704
Physical apparatus	366
Transportation and engineering	1,793
Naval architecture	802
Fisheries	10,080
Animal products	3,028
Domestic animals	162
Chemical products	1,309
Materia medica	6,317
Foods	1,111
Textiles	3,306
Forestry	726
Ethnology	423,000
Oriental antiquities and religious ceremonial	4,145
Prehistoric anthropology	153,424
American aboriginal pottery	33,293
Mammals	12,948
Birds	73,325
Birds' eggs and nests	58,041
Reptiles and batrachians	34,215
Fishes	125,000
Vertebrate fossils[1]	1,595
Mollusks (including Cenozoic fossils)	510,256
Insects	610,000
Marine invertebrates	520,000
Comparative anatomy	14,828
Palæozoic fossils	95,631
Mesozoic fossils	89,493
Fossil plants	113,685
Recent plants	252,111
Minerals	25,431
Geology	63,606
Total	3,279,531

The importance of these collections is greatly enhanced by this fact, that they include many thousands of types of the original descriptions of the pioneers of American natural history—Audubon, Baird, Agassiz, Girard, Cope, Marsh, Gray, Young, Dana, Gill, Jordan, and many more, and as such constitutes an important part of the foundation of our systematic zoology and botany.

The intrinsic value of such material as this cannot well be expressed in figures. There are single specimens worth hundreds, others worth thousands of dollars, and still others which are unique and priceless. Many series of specimens, which owe their value to their completeness and to the labour which has been expended on them, are priceless. The collections at a forced sale would realise more than has been expended on them, and a fair appraisal of their value would amount to several millions of dollars.

In the direct purchase of specimens but little money has been spent, less perhaps in fifty years than either France, England, Germany, or Austria expends in a single year on similar objects. The entire Museum is the outgrowth of Government expeditions and expositions, and of the gifts prompted by the generosity of the American people.

THE BUREAU OF EXCHANGES.

The Smithsonian system of international exchanges, begun in 1852, had for its object the free interchange of scientific material between scientific institutions and investigators in the United States and those in foreign lands. For this purpose it established correspondence with scientific societies, literary and learned men all over the world, until there is no civilised country or people, however remote, upon the surface of the planet, so far as is known, where the Institution is not represented. The list of correspondents has lengthened until those external to the country alone number nearly 17,000, while the total number is about 24,000.

[1] Only that portion of the Collection which is in Washington is included.

Many of the principal steamship companies gave generous aid in recognition of this disinterested work by granting important concessions of free freight. The United States and foreign Governments permitted the entrance through their Customs services of Smithsonian exchange boxes, and the Institution was enabled to distribute its exchange packages in this country, without expense to its funds, under the franking privilege.

In recent years the Smithsonian has been recognised by the United States Government as being in charge of its official Exchange Bureau, through which the publications of Congress are exchanged for those of foreign Governments, and by a formal treaty it acts in an official capacity as intermediary between the learned bodies and literary and scientific societies, &c., of the contracting States for the reception and transmission of their publications.

The Exchange Service has become a most valuable adjunct to educational interests, and there are few important libraries or workers in science, either at home or abroad, who have not had direct experience of its benefits.

The rules established for its control provide for the distribution to any accessible point abroad of books, pamphlets, charts, and other printed matter sent as *donations or exchanges*, and without expense to the sender beyond that of the delivery of the packages to the Smithsonian Institution in Washington, and also without expense to the receiver, except in some instances the small cost of delivery from the Smithsonian agent or correspondent nearest at hand. Similar material sent from abroad to this country is forwarded to the recipient without expense to him, the packages having been delivered free of freight charges to a foreign agent or correspondent of the Institution.

A scientific society or individual in the United States desiring to take advantage of the Exchange Service should have each of the packages transmitted strongly wrapped and separately and legibly addressed, being careful to give the full local address, and should send them in bulk, carriage prepaid, to the Institution in Washington. The separate packages should not exceed one-half of one cubic foot in bulk, and they should not contain letters or written matter.

Transmissions from abroad are received by freight in large boxes, and are distributed in the United States under frank by registered mail, a record first having been made of the name of the sender and of the address of each package. A receipt card, returnable by mail without postage, is sent with each of these packages, and should be forwarded at once by the recipient in acknowledgment of the package.

The Institution and its agents will not knowingly receive for any address purchased books, nor apparatus and instruments, philosophical, medical, &c. (including microscopes), whether purchased or presented; nor specimens of natural history, except where special permission from the Institution has been obtained.

The operations of this Bureau have affected most beneficially the libraries of all learned institutions in America. In 1867 Congress assigned to the Institution the duty of exchanging fifty copies of all public documents for similar works published in foreign countries. Finally in 1889 a definite treaty, made previously at Brussels, was formally proclaimed by the President of the United States, wherein the United States Government, with a number of others, undertook the continuation of the exchange service on a more extensive basis. Out of this has grown the Bureau of International Exchanges, for the maintenance of which Congress partially provides by annual appropriation. From 1852 to 1895 the Smithsonian exchange service handled 1,459,448 packages, and for three years past the weight of books passing through this office has been considerably over one hundred tons annually.

SPECIAL GIFTS AND TRUSTS.

The authority of the Institution to undertake the administration of financial trusts for any purpose within the scope of its general plan, preserving in connection with each fund the name of the person by whom it was established, has been recognised by Congress.

There is no institution in the world which is more favourably

situated for the administration of trusts of this character, and this privilege has, within the past few years, been accepted by several benefactors.

Dr. Jerome H. Kidder, of Washington City, bequeathed, in 1889, 5000 dols. for the purpose of an astro-physical observatory.

Dr. Alexander Graham Bell, in 1889, gave 5000 dols. to the Secretary for his personal use in physical investigation, which has been transferred by him to the credit of the Institution, and devoted to physical work.

Mr. Thomas G. Hodgkins, of Setauket, N.Y., gave, in 1891, nearly 250,000 dols., a portion of the income from which is to be applied to the investigation of atmospheric air.[1]

Robert Stanton Avery, of Washington City, who died in 1894, left property then estimated to be worth at least 50,000 dols. to provide for special investigations.

There have also been many valuable gifts to the Museum, such as that of Dr. Isaac Lea, of Philadelphia, who gave his great collections of mollusks, and of gems and precious stones; that of Mr. Joseph Harrison, of Philadelphia, consisting of the collection of Indian portraits painted by George Catlin; that of Mr. R. D. Lacoe, of Pittston, Pa., the largest existing collection of American fossil plants; and the collections of American birds' eggs given by Major Charles Bendire, U.S.A., and Dr. William H. Ralph, of Utica, N.Y.

THE ASTRO-PHYSICAL OBSERVATORY.

The Astro-physical Observatory was established in 1891, under the immediate direction of the present Secretary. The expense of maintenance has since been provided for by a small appropriation from Congress. Here is carried on work corresponding to that of similar institutions maintained by the principal European Governments, and on a much less expensive scale, though not less effectively.

Since astro-physics is almost the newest of sciences, it may not be amiss to give here a brief description of the purposes of this observatory:

"Within the past generation," we are told, "and almost coincidentally with the discovery of the spectroscope, a new branch of astronomy has arisen, which is sometimes called astro-physics, and whose purpose is distinctly different from that of finding the places of the stars, or the moon, or the sun; which is the principal end in view at such an observatory as that, for instance, at Greenwich.

"The distinct object of astro-physics is, in the case of the sun, for example, not to mark its exact place in the sky, but to find out how it affects the earth and the wants of man on it; how its heat is distributed, and how it in fact affects not only the seasons and the farmer's crops, but the whole system of living things on the earth, for it has lately been proven that in a physical sense it, and almost it alone, literally first creates and then modifies them in almost every possible way.

"We have, however, arrived at a knowledge that it does so, without yet knowing in most cases how it does so, and we are sure of the great importance of this last acquisition, while still largely in ignorance how to obtain it. We are, for example, sure that the latter knowledge would form, among other things, a scientific basis for meteorology, and enable us to predict the years of good or bad harvests, so far as these depend on natural causes, independent of man, and yet we are still very far from being able to make such a prediction, and we cannot do so till we have learned more by such studies as those in question. Knowledge of the nature of the certain, but still imperfectly understood dependence of terrestrial events on solar causes is, then, of the greatest practical consequence.

"It has been observed that this recent science itself was almost coeval with the discovery of the spectroscope, and that instrument has everywhere been largely employed in most of its work. Of the heat which the sun sends, however, and which, in its terrestrial manifestations, is the principal object of our study, it has long been well known that the ordinary spectroscope could recognise only about one-quarter, three-quarters of all this solar heat being in a form which the ordinary spectroscope cannot see nor analyse, lying as it does in the, till lately, almost unknown 'infra-red' end of the spectrum, where neither the eye nor the photograph can examine it."

This Observatory in Washington has been continuing the famous researches in regard to that invisible portion of the solar spectrum which lies beyond the limit of the red, which had been begun by Mr. Langley while director of the Allegheny Observatory. The exploration of "this great unknown region," which was first rendered possible by the invention of the bolometer, is now being carried still further by means of a new method, much perfected during the last four years, which has rendered it possible to produce a complete map by an automatic and absolutely trustworthy process, which shows the lines which resemble the so-called Fraunhofer lines in the upper spectrum. The results already attained are believed to be the most important which have ever been reached in regard to that region of the spectrum of which so little is known, and which includes the greater portion of all those energies of the sun which, through its heat, affect climate and the crops, and are thus related not only to questions of abstract interest, but to utilities of national importance.

THE NATIONAL ZOOLOGICAL PARK.

The National Zoological Park was established by Congress in 1890, as a result of the desire to secure the preservation of such American animals as are upon the verge of extinction and will soon vanish for ever if something is not done to protect them, and occupies a tract nearly twice as extensive as that of any zoological garden in the world; this includes one hundred and sixty-seven acres upon Rock Creek, only two miles north of the Executive Mansion, at the centre of the city. The site has admirable natural advantages, and much has already been done in the opening of drives and the construction of buildings.

When Congress was asked to appropriate funds for this Park, it was in view of the fact that many North American animals, constituting a part of the national wealth, and formerly occupying a large portion of its domain, are threatened with speedy extinction.

The buffalo, the beaver, the wapiti, the moose, and many other species, which until lately were abundant east of the Mississippi, are each year becoming rarer. On the Pacific Coast the sea-elephant is gone, and the walrus practically so, and the sea-otter, the fur-seal, and the sea-lion rapidly disappearing. The passenger-pigeon and the Carolina parrakeet are almost gone. It was urged upon Congress that unless steps were speedily taken, these races must perish.

The reservation of the Yellowstone National Park as a great game preserve was an important start in this direction, but the very immensity of the reservation seems to threaten the defeat of the plan, for the animals cannot be protected from marauders, and are being rapidly destroyed. To retard their extinction and to provide opportunities for their study, was the intention of those who first advocated the establishment of a preserve near Washington large enough to keep the animals as close to natural conditions as is possible, and the project seems to have been even more important than was at first supposed.

A small representative collection of native American animals has already been formed, including about five hundred individuals, among them a fine herd of young elk and a small herd of buffaloes; but the annual appropriations have not been sufficient to permit satisfactory progress.

THE BUREAU OF AMERICAN ETHNOLOGY.

The Bureau of Ethnology is an outgrowth of activities beginning early in the history of the Institution, which has from the very outset devoted much attention to the native American races.

The special work of the Bureau in its present form was begun in 1872, in response to a request from the Commissioner of Indian Affairs, who desired trustworthy information concerning the affinities of the Indian tribes, to serve as a guide in grouping them on the reservations. The question was referred by the Secretary of the Smithsonian Institution to Major J. W. Powell, then engaged, under the direction of the Institution, in explorations in the south-west. Combining the vocabularies and other manuscipts already in possession of the Institution, he prepared a report showing the character and extent of existing information, and the manner in which it was possible to utilise this in the segregation of the Indian tribes, at the same time suggesting plans for the completion of the work of classification.

This was the beginning of the Bureau, which since 1879 has been supported by special appropriations from Congress, with the understanding that the research should be so extended as to

[1] A prize of 10,000 dols., derived from this fund, was awarded August 6, 1895, to Lord Rayleigh and Prof. William Ramsay, of London, for the discovery of Argon, a hitherto unknown element in the atmosphere.

embrace the habits and customs of the American Indians, their tribal organisations and government, and their myths and ceremonials.

Major Powell was made director, and no one could have been better fitted for the task. For more than thirty years he had been a student of the native races of this continent. He and his associates in the Bureau have succeeded in placing on record, before it was too late, a vast number of facts in regard to the Indians. The Annual Reports of the Bureau, twelve in number, and nine volumes of Contributions to North American Ethnology, with the Bulletin of the Bureau, form a considerable library in themselves. The archives still contain much unpublished material, including hundreds of vocabularies.

A complete linguistic classification of the native languages of the United States has been prepared by the director, and an effective classification of the tribes on the reservation, reducing materially the danger of warlike outbreaks, has already been accomplished.

EXPLORATIONS.

The promotion of exploration has, from the beginning, been an important feature in the work of the Institution, and grants of money and loans of apparatus have been made to many hundreds of explorers, who have thus been enabled to contribute to the knowledge of the zoology, botany and ethnology of the American continent. Much has also been done in supplying scientific apparatus to the officers of the various Government surveys, which, in early days, were very often equipped only for geographical work. The Naval Astronomical Expedition to Chili was supplied by the Institution with a telescope and other apparatus, which was afterwards bought by the Government of Chili for the National Observatory at Santiago. The medical officers of the numerous surveys preliminary to the building of the transcontinental railroads, and those of the several boundary surveys, thus equipped for natural history work, made vast collections.

Members of the Smithsonian staff have frequently been detailed to serve as tidal or meteorological observers, under other departments of the Government, in remote localities, for the purposes of exploration. The important early explorations of John Xantus on the extremity of the Lower Californian Peninsula, and of Turner, Nelson, Murdoch, Kumlien, and others in the Arctic regions, were effected in this manner, as well as the earlier and more important work of Kennicott, Dall and Bannister, in Alaska, in connection with the Russian Telegraph Expedition.

On the staff of the Bureau of Ethnology important explorations of the western portion of the continent have been made, especially those of the Stevensons, Cushing, Fewkes, and the Mindeliffs, among the Pueblo people and the ruins of the south-west; those of Holmes among the prehistoric quarry sites and villages of the eastern part of the continent; those of Thomas among the mounds of the Mississippi Valley, and of McGee among the Papago and Seri Indians of the Mexican boundary; and the notable explorations of Major J. W. Powell among the tribes of Utah, California, Arizona and New Mexico.

The expeditions of Rockhill in Tibet, of Jouy in Korea, of Abbot and Chanler in Eastern Africa and Kashmir, Madagascar and the islands of the Indian Ocean, have been indirectly under the auspices of the Institution, and allusion should also be made to the visit of Major Dutton to the Hawaiian Islands for the study of volcanic phenomena, which was carried on directly at the expense of the Smithsonian Fund.

The Institution participated also in fitting out the Arctic Expedition of Kane, Hayes and Hall.

THE PROMISE OF THE FUTURE.

At the time of the Smithson bequest, the endowment of research had scarcely been attempted in America. There were schools and colleges in which science was taught, and certain of the teachers employed in these institutions were engaged in original investigation. There were a few young and struggling scientific societies, very limited in extent and influence, but at that time the chief outcome of American scientific work. Science in America was an infant in swaddling clothes. Fifty years have passed, and American science now stands by the side of the science of Great Britain, of Germany, of France, a fellow worker competing in nearly every field of research.

The Smithsonian Institution did what was, at the time of its organisation, absolutely indispensable to the rapid and symmetrical development of American scientific institutions, and but for it, science in America would no doubt have advanced with much less rapidity. It is also certain that the progress of American science has had an immense influence upon the welfare of America in every department of intellectual and industrial activity, and also a reflex action upon the scientific and industrial progress of the entire world.

This year the Smithsonian Institution will celebrate the end of its first half-century. A special volume will be published to commemorate the event, and two memorial tablets will be erected in honour of the founder in the city of Genoa, where he died, June 26, 1829; one in the English church, and one upon his tomb in the beautiful little English cemetery on the cypress-clad heights of San Benigno.

It is interesting to remember that in September next will occur not only the semi-centenary anniversary of the birth of the Institution founded in the City of Washington by Smithson, but also the centenary of the delivery of that immortal address in which Washington so forcibly recommended to his countrymen "to promote as an object of the highest importance INSTITUTIONS FOR THE INCREASE AND DIFFUSION OF KNOWLEDGE."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Council of the Royal Geographical Society offer in the present academical year a Studentship of £100, to be used in the geographical investigation (physical or historical) of some district approved by the Council. Candidates must be members of the University of not more than eight years' standing from matriculation, who have attended the courses of lectures given in Cambridge by the University Lecturer in Geography. Applications should be addressed to the Vice-Chancellor not later than the last day of the full Lent Term, March 13, 1896.

LORD HALSBURY has been elected Chairman of the Council of the City and Guilds of London Institution for the Advancement of Technical Education.

THE *Technical World* has extended its sphere of usefulness. Henceforth it will be the newspaper for secondary and technical education broadly defined. It has been accepted as the official organ of the Association of Headmasters, and of the Association of Organising Secretaries and Directors. The general policy of our contemporary will be to support the conclusions of the Royal Commission on Secondary Education, by which we understand that it will exert its influence in co-ordinating the work of secondary education. With one paragraph in the announcement of the enlargement of the journal we are in entire agreement; it is this: "That the organising secretaries and directors should have an official organ will serve to remind them (a fact which a few are prone to forget) that they have duties not only to their own counties and committees, but to each other and to higher education generally. The more they co-operate, and within limits agree, the better will be the individual work of each."

WISE words were spoken by Sir Henry Fowler last week, while commenting upon the report of the Wolverhampton Chamber of Commerce. Referring to the necessity for technical education, he remarked: "In this respect foreigners are ahead of England, and Chambers of Commerce might attach more importance to the point. In foreign countries they make greater sacrifices for it and do not grumble at the expense. In England we are now waking up to the importance of it, but we want technical education on a very much larger scale than we have as yet got it. We want it very much on the lines which the Committee of the Chamber of Commerce in their report point out, namely, technical instruction for foremen and better-class artisans. In a competition between two manufacturing countries, the country where the manufacturing population has the better technical education is more favourably placed in connection with its operations, and has a distinct advantage in the markets of the world." We are doing a little, it is true, to advance technical and scientific knowledge, but much of the money allocated to local authorities for technical education is being frittered away. Many Technical Instruction Committees are incapable of organising a scheme of instruction which will prove of permanent benefit to industry. Instead of concentrating their attention upon a few subjects, and supplying effective education in them, they devote £10 or £20 to each of a multiplicity

of little efforts. We find that technical education has been made to include all sorts of subjects, even music. Several Councils give grants to classes formed for the exclusive study of music, either instrumental or vocal. The following selection of subjects taught will give some idea of the variegated nature of "technical" work: farriery, straw-plaiting, basket-making, ploughing, draining and dyking, clicking, cabinet-making, thatching, sheep-shearing, fishing, sail-making, china-painting, hat-manufacture, type-writing, political economy, life-saving, and house-decoration. So long as such subjects absorb the attention of Committees, little national advancement is possible. Instruction in the dodges of the workshop may produce a more dexterous and quicker workman, but it does nothing to educate him in those broad principles which enable him to assist intelligently in the real improvement of industry.

THE spirit of rivalry which regulates the conduct of educational institutions in some of our large towns is to be deplored, for its effects are detrimental to the advance of education. Reports, received from time to time, show that, in many districts, local institutions compete with one another instead of forming distinct steps in the educational ladder. So common is this kind of competition that it is refreshing to learn that the City Council and the School Board of Manchester have agreed between themselves that the Technical School shall discontinue its more elementary classes, and begin its curriculum at the points where the Board schools leave off. An effort is to be made to secure a corresponding gradation between the Technical School and Owens College. Manchester has thus taken important steps towards the solution of a difficult problem in public education, and it would be well if those provincial towns that have not already considered the correlation of their technical and scientific institutions would do so without delay. It is a question, indeed, whether a central authority ought not to be able to give a definite place in the educational ladder to the various institutions in a town, and to insist upon the absence of competition with one another. With each part of the engine doing its proper work, progress will be made; but if there is a confusion of functions, advance is impossible. The establishment, in recent years, of numerous technical institutions in many of our large provincial towns, and the extension of the work of old-established Mechanics' Institutes and Trade Schools, make it very necessary that something should be done to define the place of these institutions in our educational system. The University Colleges are especially affected by such institutions. Bristol, for instance, possesses one of the best University Colleges in the country; it has done excellent work, and will certainly do more. But during the past few years the Merchant Venturers' School has largely developed, and it is now a rival establishment situated only a few hundred yards from University College, with which it competes. This competition is no doubt responsible, to some extent, for the adverse balance of £950 in the accounts of University College, Bristol, for the year 1895; the total indebtedness of the College is now more than £6000. There is ample room for both institutions in Bristol, but the work of one should supplement, and not clash with, the work of the other. What is happening in Bristol is happening elsewhere, and is retarding educational advancement. In fact, we have no hesitation in saying that one of the most important points which needs to be settled at the present time is that which refers to the status of various institutions in the scheme of education.

SCIENTIFIC SERIALS.

American Journal of Science, January.—The quarries in the lava beds at Meriden, Conn., by W. M. Davies. The present condition of the quarries in the Triassic (Newark) formation near Meriden shows the vesicular upper surface of one lava bed under the dense basal portion of a later flow, and a number of fractures dislocating the double flow.—The form of isolated submarine peaks, by G. W. Littlehales. Theoretically the form of an isolated submarine peak would be that of a solid of revolution in which the crushing strength of any section is equal to the combined weight of the portion of the formation above that section and of the superincumbent body of water. The author derives a general equation for the slope of submarine peaks, and finds that the average slopes of Dacia Bank, Seine Bank, The Salvages, and Enderbury Island are fairly in accordance with the formula. This investigation has an important bearing upon the intervals at which deep-sea soundings should be taken in searching for probable shoals in the open ocean and in developing the character of the sea-bottom. The minimum radius at the bottom which a dangerous shoal can have, must vary directly with the depth, but on the average, in the deep sea, it may be stated as ten miles. An interval of ten miles, coupled with an interval of two miles, would be sufficient for general development, and would prove with certainty the existence or absence of any formation rising close to the surface.—On the epidote from Huntingdon, Mass., and the optical properties of epidote. This epidote is almost identical with that of Zillerthal, in Tyrol, but has the lowest percentage of iron oxides (6·2) and the lowest double refraction of any epidote recorded.—The iodometric determination of selenious and selenic acids, by F. A. Gooch and A. W. Peirce. The principle previously applied to the estimation of chlorates is equally advantageous for the determination of selenious and selenic acids. The selenious acid is treated with potassium iodide, di-hydrogen potassium arseniate, and half-strength sulphuric acid. The liquid is concentrated by boiling, the residue is cooled and the acid nearly neutralised with potassium hydroxide, acid potassium carbonate is added in excess of neutralisation, and, after the addition of starch, standard iodide is introduced until the starch-blue appears. The iodine introduced measures the arsenious acid, and the difference between it and the iodine, originally present in the form of the iodide, represents the amount set free by the selenious acid. Selenic acid, on the other hand, may be determined iodometrically with accuracy by first reducing it to the condition of selenious acid by treatment with potassium bromide in the presence of sulphuric acid, and then completing the reduction to the elementary condition by the treatment with potassium iodide and potassium arseniate.

SOCIETIES AND ACADEMIES.

LONDON.

Geological Society, December 18, 1895.—Dr. Henry Woodward, F.R.S., President, in the Chair.—Prof. G. K. Gilbert, Washington, D.C., was elected a Foreign Member, and Dr. A. Penck, Vienna, was elected a Foreign Correspondent of the Society.—The tertiary basalt-plateaux of North-western Europe, by Sir Archibald Geikie, F.R.S. The author in this paper gave the results obtained by him in the continued study of Tertiary volcanic geology during the seven years which have elapsed since the publication of his memoir on "The History of Volcanic Action during the Tertiary Period in the British Isles." His researches have embraced the Western Islands of Scotland, St. Kilda, and the Faröe Islands. In an account of the rocks of the basalt-plateaux, attention was particularly directed in this paper to a type of banded basic lavas which played an important part in the structure of the volcanic districts both of the Inner Hebrides and of the Faröes. A number of examples were adduced of the volcanic vents which form a characteristic feature of the basalt-plateaux. The paper described in some detail the evidence for the flow of a large river across the lava-fields during the time when volcanic activity was still vigorous. Many additional details were given to illustrate the structure and behaviour of the basic sills which are so abundantly developed, especially at the base of the plateaux. The author added some additional particulars, more especially from Skye and St. Kilda, to his published account of the dykes which had taken so important a place in the origin and structure of the plateaux. Further observations were narrated regarding the great bosses of gabbro in the Inner Hebrides. The author, having been able to visit St. Kilda, described the junction of the granophyre of that remote island with the basalts and gabbros. He brought away a series of specimens and photographs which demonstrated that the acid rock had been injected into the basic masses, traversing them in veins and enclosing angular pieces of them. The granophyre was precisely like that of Skye and Mull, and was traversed by veins of finer material, as in these islands. By way of illustrating the probable history of the basaltic plateaux of North-western Europe, the author gave a short summary of the results of recent investigations of the modern volcanic eruptions of Iceland, especially of Th. Thoroddsen and A. Helland. Reference was made to the evidence of considerable terrestrial movement since the Tertiary volcanic period, as shown by the tilting of large sections of the plateaux in different directions, and also by the existence of actual faults. The con-

cluding section of the paper dealt with the effects of denudation on the plateaux. The author remarked that there was certainly no other area in Europe where the study of the combined influence of atmospheric and marine denudation could be so admirably prosecuted, and where the imagination, kindled to enthusiasm by the contemplation of such scenery, could be so constantly and imperiously controlled by the accurate observation of ascertainable fact.—The British Silurian species of *Acidaspis*, by Mr. Philip Lake. Descriptions were given of those species of *Acidaspis* in the Silurian of Britain which have hitherto been incompletely described. The British forms were compared with those from the same system in Sweden and Bohemia. Five, out of nine, were represented by the same or very closely allied species in Sweden; two in Bohemia. All the Swedish forms except one were represented in Britain, and one in Bohemia as well as in Britain.

Royal Microscopical Society, December 18, 1895.—Mr. A. D. Michael, President, in the chair.—Mr. E. M. Nelson exhibited and described a portable microscope in which the stage had been enlarged to $4\frac{1}{2}'' \times 5''$, and the body fitted with three draw-tubes giving a range of length from $4\frac{3}{4}''$ to $12\frac{3}{4}''$.—A discussion on tube-length ensued, in which Mr. C. Beck, Mr. Nelson, and Mr. J. E. Ingpen took part.—Dr. H. C. Sorby gave an interesting account of his methods for preserving some of the more delicate marine organisms.—Mr. T. D. Ersser exhibited a new method for showing the multiplied images formed by the compound eyes of insects.—The President having reminded the Fellows that the meeting on January 15 would be their annual meeting, the list of Fellows recommended as officers and Council for the ensuing year was then read.

Mathematical Society, January 9.—Major Macmahon, R.A., F.R.S., President, in the chair.—Prof. Elliott, F.R.S., by a method used in connection with seminvariants, showed how to obtain a criterion as to whether or not a rational integral homogeneous function of y, a function of x, and its derivatives, is an exact differential, and further showed that if it is its integral can be found by differential operations only.—The President announced the title of a paper by Prof. Tanner, viz. on a certain ternary cubic. The paper, in the absence of the author, was taken as read. The notes chiefly relate to the automorphs and units of the form, and include a short geometrical discussion.—Mr. S. H. Burbury, F.R.S., made a further communication on Boltzmann's minimum function. Lieut.-Colonel Cunningham, R.E., and Dr. Larmor, F.R.S., joined in a discussion on the paper.—Mr. Love, F.R.S. (Hon. Sec.), communicated some examples illustrating Lord Rayleigh's theory of the stability and instability of certain fluid motions, and subsequently answered questions, bearing on the subject, by Dr. Larmor.

Entomological Society, January 15.—The sixty-third annual meeting, Prof. Raphael Meldola, F.R.S., President, in the chair.—After the balance-sheet had been read by one of the auditors, Mr. Goss read the report of the Council. It was announced that the following gentlemen had been elected as officers and Council for 1896: President, Prof. R. Meldola, F.R.S.; Treasurer, Mr. Robert McLachlan, F.R.S.; Secretaries, Mr. Herbert Goss and the Rev. Canon Fowler; Librarian, Mr. Geo. C. Champion; and as other members of the Council, Mr. Walter F. H. Blandford, Mr. Geo. F. Hampson, Prof. Edwd. B. Poulton, F.R.S., Mr. Osbert Salvin, F.R.S., Dr. D. Sharp, F.R.S., Mr. Roland Trimen, F.R.S., the Lord Walsingham, F.R.S., and Colonel J. W. Yerbury, R.A. It was announced that the President would appoint Dr. D. Sharp, Mr. Roland Trimen, and Mr. W. F. H. Blandford Vice-Presidents for the Session 1896–1897.—Prof. Meldola then delivered an address, in which he first drew attention to the remarkable literary activity of the entomologists of this country during the past year, referring particularly to the works recently published by Miall, Meyrick, Barrett, Rye, Lucas and Buckton, and to the new volume of the "Cambridge Natural History" by Sedgwick, Sinclair and Sharp. Attention was also called to the interesting discoveries in insect physiology by Latter and Hopkins. The main portion of the address was devoted to a plea for a more liberal use in biological work of the theoretical or speculative method which had proved so fruitful in other branches, and which, in the President's opinion, might with advantage be more freely employed in connection with entomological investigation. Illustrations were taken from the work of Bates on mimicry, Wallace on the colours of insects, and Poulton's researches on variable colouring, all of which had been prompted by hypothesis, and which had led to discoveries of large bodies of facts which would never have been gleaned by haphazard observation. In conclusion, the President referred to the losses by death during 1895 of many Fellows of the Society and other entomologists, special mention being made of Prof. Charles V. Riley, Prof. C. C. Babington, F.R.S., the Right Hon. T. H. Huxley, F.R.S., M. E. L. Ragonot, Major J. N. Still, Prof. Carl E. A. Gerstäcker, M.D., M. Claudius Rey, M. Jules F. Fallou, and Mr. W. H. Tugwell.

Royal Meteorological Society, January 15.—Annual meeting.—Mr. R. Inwards, President, in the chair.—The report of the Council showed that the Society was in a satisfactory condition, thirty-four new Fellows having been elected during the year. Mr. Inwards devoted his presidential address to the subject of meteorological observatories, which he illustrated with numerous lantern slides. After describing some ancient observatories, including the Nilometers and the Tower of the Winds at Athens, he gave an account of national observatories, of which the Royal Observatory, Greenwich, was taken as a type. High-level observatories were next described, of which that on Mont Blanc was taken as a type. Special reference was also made to the observatories on the Sonnblick, the high-level observatory at Arequipa on the Andes, and that on Ben Nevis. An account was next given of tower observatories, together with some of the results obtained from the Eiffel Tower at Paris. Mr. Inwards, in concluding, said: "One can figure to oneself a tower piercing the air from any of the elevated tablelands of this country—Salisbury Plain, the Stray at Harrogate, or the Downs between Guildford and Dorking—and from which the most interesting results could not fail to accrue. It is the opinion of M. Vallot—no mean authority—that a high tower is for air-observing purposes equivalent to a mountain station of ten times the altitude: and this is plain when one considers that any mountain must act as an obstacle which thrusts upward the strata of the atmosphere into a form almost like its own, so that some of the effects are very little different from those observed below; while a tower like the Eiffel Tower thrusts itself in the air without obstructing its movements. It is the boast of the Royal Meteorological Society that it is gradually covering the country with a network of private observing stations, and is collecting together, for the enlightenment of all future time, a mass of accurate knowledge on the subject of the changes in our atmosphere, its varying moods, its beating pulses, its calms and its convulsions, so that when the philosopher is born who is destined to unravel all its mysteries, he will have the tools and instruments ready to his hand."—Mr. E. Mawley was elected President for the ensuing year.

EDINBURGH.

Royal Society, December 2, 1895.—Prof. Geikie in the chair.—Before business proper was commenced, the Chairman, in reviewing the work of the past session, congratulated the Society on the reappointment of Lord Kelvin as President. This was the third time, he remarked, that the Edinburgh Royal Society had provided the Royal Society of London with a President. In referring to the successful completion of the *Challenger* Reports, for which the Society offered its heartiest congratulations to Dr. Murray, Prof. Geikie hoped that Government would be induced, by the publication of these results, to equip a proper expedition to the Antarctic regions.—Prof. Tait read a paper, by Lord Kelvin, on the application of network to a surface, in particular to a toroidal surface. Lord Kelvin had had his consideration directed to this subject, in connection with attempts to protect the pneumatic tyres of bicycles, which were examples of a toroidal surface. Dr. Noel Paton read a communication on the relationship of the liver to fats.—Mr. James Milne, gave an account of a mass of manufactured iron, which he found in the valley of the Rhone near the Glacier de la Plaine Morte. It bore the date 1807, and Mr. Milne was of opinion that it had been left there as a mark by an expedition which did some scientific work in the Alps in that year.

December 16, 1895.—Prof. Copeland, Vice-President, in the chair.—An obituary notice of the late Dr. Benjamin Carrington, by W. H. Pearson, Esq., was read by Prof. Tait.—Dr. C. Hunter Stewart, of the Public Health Laboratory, made two communications on allied subjects: Three years' daily determinations of the amount of carbonic acid in the atmospheric air, and in the ground air of Edinburgh; and on the physical and chemical examination of the soil, and the relation of the soil to the incidence of summer diarrhœa in Scotland.—

The result of his investigations on the first subject showed that the proportion of CO_2 in the atmosphere of Edinburgh in 1893 was 3·96, in 1894, 3·72, and in 1895, so far, 3·45 per ten thousand. Dr. Stewart contended that eventually the amount of CO_2 in the air depended on the nature of the soil, as the ground air was the source and regulator of the atmospheric. The ground soil of Edinburgh being clay, produced less carbon than a "travelled" and therefore porous soil. Passing to the second part of his subject, Dr. Stewart adduced statistics to show that, in the case of summer diarrhœa, the mortality was low when the soil was clay, and high when it was sandy.—Prof. Hartley, of Dublin, read a paper on the cause and nature of chemical changes in ocean deposits, and Dr. J. L. Kerr described a new method of making plaster of Paris.

PARIS.

Academy of Sciences, January 13.—M. A. Cornu in the chair.—Observations of the minor planet CH Charlois (January 8), made at the Observatory of Paris, by M. O. Callandreau.—The expenditure of energy in muscular work, by M. A. Chauveau. In the case of a muscle which is doing positive work, such as lifting a weight, the expenditure of energy by the muscle is divided by the author into two parts; one spent in displacing the weight, the other in sustaining it during the displacement. In the experimental study, the external work done in lifting a weight is measured directly, the energy changes in the muscles indirectly, by means of analyses of the inspired and expired air.—Note from the report of M. P. Ballif on the hydraulic works carried out in Bosnia-Herzegovina, and on the meteorology of the two provinces, by M. B. de la Grye.—On the variations in the ratio of the two specific heats of gases, by M. E. H. Amagat. From calculations of the values of the ratio of the specific heats of air at 50° C. under pressures varying from 1 to 50 atmospheres, widely differing results are obtained according as the determinations of the specific heat at constant volume by Joly, or those at constant pressure by Lussana, are taken as the basis of the calculation. The results of Joly are shown to be the most probable. The expression $p(v - \epsilon)^\gamma =$ constant, where ϵ is $\frac{d(pv)}{dp}$ and p, v, and γ have their usual significations, is then developed, as giving the adiabatic expansion for fluids, including highly compressed gases. For low pressures ϵ is negligible, and the equation reduces to the ordinary form.—Observations of the minor planet CH Charlois, made at the Observatory of Toulouse, by M. F. Rossard.—On the nature of the solar prominences, by M. J. Fényi.—On the generalisation of the idea of the limit, and on the extension, to summable divergent series, of Abel's theorem on complete series, by M. E. Borel.—On the theory of cathode rays, by M. G. Jaumann. A reply to a note of M. H. Poincaré. The author thinks it is not necessary to modify his hypotheses to meet the objections of M. Poincaré.—Observations on the preceding communication, by M. H. Poincaré.—On Hall's phenomenon in liquids, by M. H. Bagard. An experimental proof of the existence of the Hall effect in a saline solution. The results for a solution of zinc sulphate are of the same order of magnitude as for metallic bismuth. The experiments contradict the conclusion previously arrived at by M. Roiti, that Hall's effect did not occur with liquids.—Acetylene as a photometric standard, by M. J. Violle. As a single chemical individual of high illuminating power now easily obtained in a state of purity, acetylene offers obvious advantages as a secondary photometric standard. A flat flame of acetylene burning under a pressure of 30 cm. of water, and used with a screen, gave perfectly satisfactory results.—On the heat of formation of some compounds of manganese, by M. H. Le Chatelier. Thermochemical data are given for the combustion in oxygen of manganese, its carbide and protoxide.—On the crystallised iodides of strontium and calcium, by M. Tassilly. A thermo-chemical paper.—On aldehydes derived from the isomeric alcohols $C_{10}H_{18}O$, by MM. Ph. Barbier and L. Bouveault.—On the multirotation of the reducing sugars and isodulcite, by M. Tanret.—Retinal oscillations resultingfrom luminous impression, by M. A. Charpentier.—On the formation of the duramen, by M. E. Mer.—On a new locality in France for *Pinus Salzmanni*, by M. G. Fabre.—Pliocene glaciers in the mountains of Aubrac, by M. G. Fabre.—On some anomalies in the temperature of subterranean springs, by M. E. A. Martel.—On the mechanical production of extreme temperatures, by M. E. Solvay. A note admitting the priority of Prof. Dewar in the use of vacuous envelopes for retarding heat radiation.—On a meteor observed at Chambéry, by M. Chabert, January 6, 5.15 p.m.; direction N.E. to S.W.

BERLIN.

Meteorological Society, December 3, 1895.—Prof. Hellmann, President, in the chair.—General von Tillo, of St. Petersburg, who was present as a guest, explained a series of charts giving the chief results of his observations on the distribution of terrestrial magnetism.—Dr. Fischer described in detail a thunderstorm which occurred over Silesia and Posen on September 30. It began at midnight in a region of low temperature, advanced very slowly, accompanied by very light or even no wind, and did not cease until the next night. The storm area was covered with dense mist, rain was slight and local, and the atmospheric pressure was maximal. This storm was therefore due to neither heat nor wind eddies.—Dr. Zenker spoke on the climate of Werchojansk, with special reference to its temperatures.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Plant-Breeding: L. H. Bailey (Macmillan).—N.S.W. Government Railways and Tramways. Annual Report of the Railway Commissioners for the Year ending June 30, 1895.—First Annual General Report upon the Mineral Industry of the United Kingdom for the Year 1894: Dr. C. Le Neve Foster (Eyre).—On the Deep and Shallow-Water Marine Fauna of the Kerguelen Region of the Great Southern Ocean: Dr. J. Murray (Edinburgh, Grant).

PAMPHLETS.—Von der Menschlichen Freiheit: Dr. H. Achter (Leipzig, Engelmann).—Review of the Mineral Production in India for 1894 (Calcutta).

SERIALS.—Quarterly Journal of Microscopical Science, January (Churchill).—American Journal of Psychology, Vol. vii. No. 2 (Worcester, Mass.).—Psychological Review, January (Macmillan).—North American Fauna, No. 10 (Washington).—Journal of the Franklin Institute, January (Philadelphia).—Journal of the Sanitary Institute, January (Stanford).—Palestine Exploration Fund Quarterly Statement, January (Watt).—Intles. Archiv für Ethnographie. Band viii. Heft 5 and 6 (Leiden, Brill).—Record of Technical and Secondary Education, January (Macmillan).—Nyt Magazine for Naturvidenskaberne. 34te Binds 3 die og 4 de Hefte, 35te Binds, 1 ste, 2 de og, 3 die Hefte (Christiania).

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